THE JOURNAL OF WILDLIFE MANAGEMENT

VOLUME 15 JULY, 1951 NUMBER 3

POPULATION CYCLES AND RANDOM OSCILLATIONS

LaMont C. Cole

Department of Zoology, Cornell University

INTRODUCTION

Everyone who has had a few years of experience in field work is aware that natural populations of animals, and frequently of plants, vary in size from year to year. These numerical fluctuations are often extreme and suggest to the ecologist that an insight into their causes may be of fundamental importance for understanding the interactions between environmental factors and populations of organisms. Modern evolutionary theory and recent trends in genetics are also emphasizing population size as a field deserving study in its own right (*cf.* Allee, *et. al.* 1949) so there is ample justification for the wealth of literature devoted to population changes. Moreover, in approximately the last 50 years (*cf.* MacFarlane, 1905) there has come into existence a belief that great regularity characterizes the "cycles" of population size and that the explanation for this regularity must be sought among environmental factors exhibiting rhythmic fluctuations. It is not our purpose to review the vast literature concerned with periodic population fluctuations which has accumulated, especially since the appearance of Elton's classic paper (Elton, 1924). For a thorough review of the entire subject the reader is referred to Leopold (1933), MacLulich (1937), Clements and Shelford (1939), Elton (1942), Dymond (1947), Siivonen (1948), and Clarke (1949).

If one is justified in concluding that populations do exhibit regular, predictable fluctuations in time the subject is of much more than theoretical interest because it suggests possibilities for predicting future outbreaks of noxious organisms and future yields of fish, game, and fur-bearers. Various opinions about the regularity of population cycles have been expressed ranging from acceptance of precise regularity to the recent suggestion by Palmgren (1949) that the observed population "cycles" may conceivably be regarded as random fluctuations. Accordingly, it seems that some re-examination of the hypothesis of regularity should be profitable.

A scientific approach to the analysis of any empirical phenomenon constrains us to be governed by the so-called "law of scientific parsimony"; that is, to avoid more complicated hypotheses than are actually required to explain the observed facts. If Palmgren's suggestion is tenable; if the observed population fluctuations can be regarded as random oscillations or otherwise explained on the basis of simple assumptions, we must regard all more complicated hy-

potheses which have been proposed as premature in terms of our present state of knowledge.

The usual approach to a study of population oscillations is to take a series of population estimates made in successive time intervals, usually years, plot these estimated sizes on graph paper, and attempt to decide whether regular oscillations are indicated. It is in reaching this decision that one may easily be misled. Population data, unfortunately, are likely to provide a discouragingly short series of heterogeneous measurements of somewhat questionable accuracy and to suggest a certain amount of variability in the lengths of the intervals between the successive maxima or minima of population size. If, despite these limitations, one undertakes the formidable computational tasks of resolving the series of numbers into a sum of periodic mathematical functions, it is uncertain whether two workers employing the same data will emerge with the same assortment of equations. This type of analysis is difficult enough when dealing with such regular cyclic phenomena as alternating current voltages or tidal movements; with the more complex time series which usually interest the ecologist the situation may become almost hopelessly complex.

The student of populations may seek to avoid the difficulties involved in analyzing time series by attempting to isolate from his data cyclic movements which exhibit synchronization with broad "climatic cycles" which the biologist quite naturally learns about from workers in other fields of investigation. However, these cosmic rhythms may quite possibly have been derived from confusingly variable data by processes no more conclusive than those applied to the population data. A prominent student of climatology could write in 1941, after referring to a listing of more than 50 "climatic cycles":

> Though firm advocates of climatic cycles will sharply disagree, such facts as we possess today neither definitely demonstrate nor disprove the existence of any real cycle. Such climatic variability as has been observed may be explained as resulting wholly from random fluctuations. (Russell, 1941).

It might also be remembered in this connection that one of the arguments used by MacLulich (1937) in discounting the coincidence of the Canadian lynx population cycle and the sunspot cycle is the greater regularity of the lynx cycle which has varied from "eight to twelve years" while the sunspot cycle has varied between "seven and seventeen years."

If the biologist recognizes these difficulties he may be reduced to reliance on subjective impressions regarding the "cyclic" variations in his data, a precarious procedure described by a statistician in the following words:

> Experience seems to indicate that few things are more likely to mislead in the theory of oscillatory series than attempts to determine the nature of the oscillatory movement by mere contemplation of the series itself; . . . . (Kendall, 1946).

Some of the difficulties of interpreting time series may be noted by reference to Figure 1, which shows population fluctuations of the arctic fox (*Alopex*) and of the wolf taken from Hewitt (1921), and fluctuations in the annual growth made by Douglas Fir trees in Utah taken from Schulman (1948). In order to obtain a comparable random series we have started on page one of

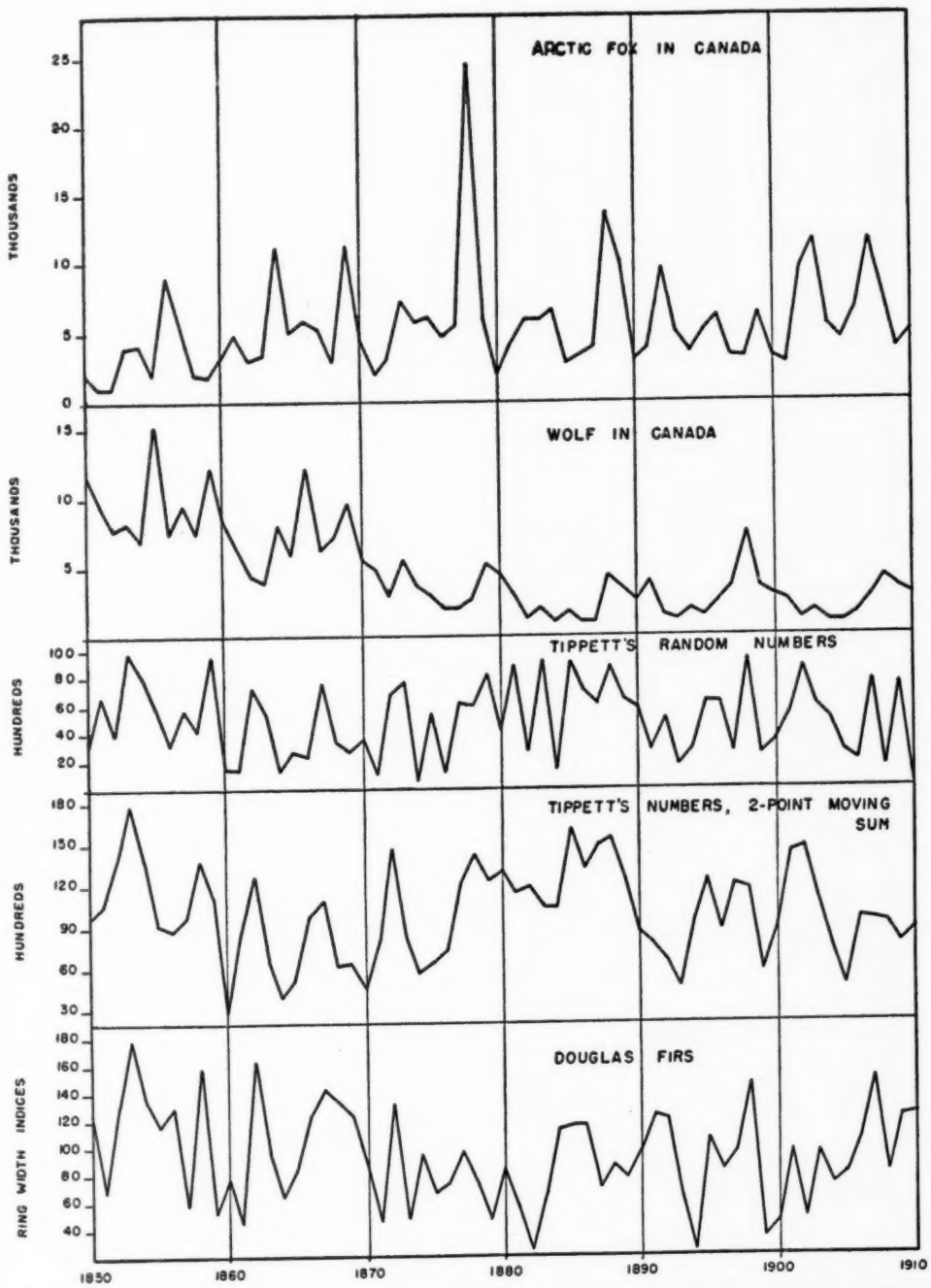

FIG. 1. Fluctuating series compared: arctic fox and wolf from Hewitt (1921), random sampling numbers from Tippett (1927), and Douglas fir growth from Schulman (1948).

the most venerable of the several available sets of "random sampling numbers" designed for statistical sampling experiments (Tippett, 1927), and have plotted these random numbers, taking them in the order in which one would read an ordinary book. Also shown is a two-point moving sum of these same numbers (first number plus second number, second number plus third number, etc.) —a device that permits the height of each point on the graph to depend upon the values of two successive random numbers much as we might expect current population size to be affected not only by conditions this year but also by the size of last year's population. Examining Figure 1, it appears to the writer that Tippett's numbers, which have been subjected to and have successfully passed a large number of tests for randomness, compare favorably with the biological data in apparent regularity of the oscillations. As for the possibility of erroneously postulating a connection between independent time series, Tippett's numbers (especially the two-point sum) and the Douglas Fir growth indices offer a striking example which should serve as a warning for all workers handling data of this sort. Even very critical investigators, when confronted with two time series as similar as these completely independent series are for the first 25 years illustrated, might find it very difficult to resist the assumption of some causal connection between the two.

In view of the uncertainties and chances for error involved in attempting to analyze and interpret such complex time series as population data usually provide, it appears to the writer that we should adopt a fresh approach; start with the simplest conceivable explanations for population cycles, deduce the consequences of the mechanisms we postulate, and compare the results with the observed data. If our simple explanations do not suffice to explain the facts, we shall at least have gained a conception of the degree of complexity required for any adequate explanation. To the best of the writer's knowledge Palmgren (1949) is the only worker who had adopted this general approach in dealing with population cycles. By rolling dice and drawing numbered cards from a well-shuffled deck, Palmgren found that he obtained average intervals between peak values which accorded well with the average intervals deduced by Siivonen (1948) from the study of population data. The implication is that years of peak populations might appear purely at random and this would seem to point to about as simple a conception of population cycles as could be hoped for. Our purpose here is to extend Palmgren's approach by attempting to decide what features of the observed population fluctuations are established with sufficient certainty to merit comparison with theoretical schemes and then to make the appropriate comparisons by deducing the results to be expected under some of the simplest conditions for population fluctuations.

TERMINOLOGY

In the mathematical sense a fluctuating series of up and down phases is in general designated as "oscillatory" while the term "cycle" is reserved for recurrent oscillations of such regularity or periodicity that values are predictable far into the future (see Kendall, 1946). In this strict sense, population fluctua-

tions are oscillatory rather than cyclic. However, "population cycle", "sunspot cycle", "business cycle", and the like, have become well established terms for phenomena lacking precise regularity and it would doubtless be futile to insist upon strict terminology. It is, however, confusing to find the terms "regular cycle", "periodic cycle", or even "regular, periodic cycle" applied to oscillatory phenomena which lack precise regularity. For example, Grange (1949) states (p. 119) ". . . . a cycle can show intervals of 3, 4, 5 or other differing numbers of years and still be a 'regular' cycle," and further (p. 122) he indicates that a cycle may be regarded as a "regular" ten-year cycle although the high points and the low points (peaks and troughs) are allowed a variation of two years either before or after the specified year. This loose terminology which evidently permits a "regular" ten-year cycle to vary in length anywhere from two years to eighteen years suggests that any explanation for population cycles need only account for some rough average length of the interval between successive oscillations. Possibly this usage of the terms "regular" and "periodic" has caused widespread misconceptions regarding the nature of population fluctuations.

THE CHARACTERISTICS OF THE OBSERVED CYCLES

Upon examining the literature on population cycles one may list a number of features which should offer clues as to the type of mechanism involved. Prominent among these are 1) the types of animals involved; 2) the degree of synchronization of the cycles in different localities; 3) the nature of the upward and downward phases, whether gradual or abrupt, and the amplitude of the oscillations or difference in population level between peak and trough years; 4) the interval or average length of time required for one complete oscillation; and 5) the degree of constancy or variability of this average cycle length. However, on some of these points we cannot consider our information definite enough to justify attempts to set up theoretical models.

As to the types of organisms involved, some writers consider that the cycles are mostly confined to high latitude forms, particularly small mammals, gallinaceous birds, and certain fish such as salmon. However, insects in various parts of the world (Eidmann, 1931, Uichanco, 1936, Shelford and Flint, 1943) rate of tree growth (Huntington, 1945, Siivonen, 1948), and even disease epidemics (Budai, 1930) have been postulated as showing the same cycles, while Peterson (1947) even claims to find a reflection of sunspot cycles in such human events as revolutions and births of geniuses and twins. Predators such as owls, hawks, foxes, and lynxes that can breed rapidly enough to exploit the expanding food supply during an upward population phase in their prey also exhibit cycles, but these are of a secondary nature and leave us with the basic problem of explaining the cycles in the prey populations. It is self evident that many larger animals, such as bear and moose, with their low reproductive potentials could not possibly exhibit very dramatic population growth within time intervals on the order of five years and hence could not exhibit the short term cycles described for the smaller forms. Thus the common ob-

servation that the cycles are confined to species with high biotic potentials does very little to clarify the picture.

The degree of synchronization between different regions is uncertain; in the words of Clarke (1949): ". . . . at present the basic conflict of testimony may be said to be more impressive than the explanatory theories." If the cycles could be shown to occur simultaneously in different regions more frequently than we are willing to ascribe to chance it would, as Elton has contended, provide evidence for control of the cycles by widespread influences, presumably meteorologic. However, meteorologists seem reluctant to commit themselves as to how closely weather conditions in different regions may be expected to be correlated, and the analyses of MacLulich (1937) and Cross (1940) cast doubt on the synchronization of the population cycles. More precise data are evidently required before this question can be solved.

Unfortunately, no unequivocal statement can be made about the shape and amplitude of the population oscillations from published data. The most nearly adequate series of records have been obtained from fur returns where economic considerations, variations in the amount of trapping, and other such disturbances make it very unlikely that the published figures can be regarded as proportional to total population size. In other cases questionnaires, subjective impressions, recollections of taxidermists and old settlers, and similar sources have been used to date the up and down phases of the cycles, but such sources provide nothing like absolute population estimates. The studies involving careful census techniques will not be of adequate length for some time to come. In the case of tree ring widths, corrections can be made for age and species of tree, apparent long time trends, and other variables to provide indices which actually represent relative annual growth rates over long periods (see, for example, Schulman, 1942). However, we have no real reason to expect the amplitude of cycles in these indices to parallel fluctuations in animal populations, so it does not seem worthwhile to attempt any quantitative conclusions about the amplitude of population cycles.

One point regarding the shape of the population oscillations does require discussion. This concerns the definition of a peak population year. Logically it seems that any year in which the population is greater than in the year preceding and in the year following should be regarded as a peak in the cycle, but to do so one must resist the temptation to consider only the extremely high peaks. Thus differences of opinion may arise as to how many cycles are included in a given series of observations. For example, Elton and Nicholson (1942), discussing the lynx cycle in Canada, find a mean distance of 9.6 years between peaks. However, if one examines their corrected table of the figures originally published by Poland, and counts every peak year including little peaks for the period 1751-1889, 23 "cycles" with an average length of 5.8 years are found between the first peak in 1752 and the last in 1886.

From what is known of the general pattern of population growth (see Allee *et. al.* 1949) the present writer does not believe that we should ignore little peaks. Both in theory and by actual

observation, a small population (one which is far below the immediate "carrying capacity" of its environment) tends to grow at compound interest. Thus it is a self-accelerating process becoming more rapid as the population becomes larger. If we imagine some environmental circumstance of a particular magnitude which appears in certain years and induces the population to grow, we realize that this factor may in one year produce a little peak if it operates on a small population which does not have time to reach a size where "interest" is accumulating rapidly. In another year, however, the very same impetus might produce a very much higher peak if it came to bear on a slightly larger initial population. This point will be readily understood if one considers the difference in absolute gain in the same length of time to be obtained from two slightly different sums of money growing at compound interest and if one remembers that the small rodents which are the basis of the most discussed population cycles have potential "compound interest rates" on the order of several hundred per cent per season. Consequently, we shall here define a peak as any year in which the population exceeds that of the two adjacent years.

This brings us finally to the subject of the average distance between peaks and the amount of constancy of this average figure as a basis for comparing hypothetical and observed series of population cycles. It is generally considered that there are two common cycle lengths in animal populations; a three to four year cycle of the lemmings and other "mouse-like" rodents and a nine to ten year cycle especially in the snowshoe rabbit (*Lepus americanus*). Predators follow the periodicity of their prey species; the lynx, among others, reflects the ten year cycle of the rabbit, and the arctic fox (*Alopex lagopus*) and the snowy owl (*Nyctea scandiaca*) follow the four year period of the lemmings. The colored foxes (*Vulpes fulva*) and the marten (*Martes americana*) are said by Dymond (1947) to have a four-year cycle in the arctic and a nine- to ten-year cycle farther south, reflecting differences in their basic prey animals in different regions. With very few exceptions the cycles described for other species are believed to fit in with one or the other of these periodicities.

Elton (1924) suggested a connection between the three-year and ten-year cycle lengths, but this possibility seems to have been largely neglected until Siivonen (1948), after patiently scrutinizing a vast amount of data, reached the conclusion that the ten-year periodicity represents an average distance between "high peaks" and that the basic cycle is one of three to four years in length, averaging 3 1/3 years. He found three years to be the most common interval between peaks and a tendency for every third peak to be a "high peak."

Palmgren (1949) has questioned some of Siivonen's assumptions in arriving at average cycle lengths and has obtained slightly different average lengths using the same data. However, the differences are not great and Palmgren does not present the relative numbers of cycles of different lengths according to his interpretation. This merely serves as one more warning that a subjective element is involved in interpreting the data now available on population cycles.

Table 1 shows the frequency distributions of cycle lengths for some of the populations where a relatively large number of cycles has been observed. Except for the first three columns the data are taken from Siivonen's Table 34. Poland's data on the Canada lynx (as corrected by Elton and Nicholson, 1942) are included despite the small number of cycles to contrast with the data on the lynx in Finland. The first two columns represent tree growth as determined from corrected ring widths, the Utah Douglas Fir records being taken from Schulman (1948) and the Pennsylvania Hemlock data from Meyer (1941). Siivonen's data do not really represent observations over the great span of years which might be inferred from the large number of recorded cycles, but were obtained by lumping together different regions so that a widespread cycle which was recorded in several locations might be counted several times in the table. On the other hand, data such as those on the Canada lynx refer to general collections of pelts over a wide area so that cycles in two regions might, if they happened to be out of phase, tend to cancel each other out. The tree ring data provide the only really extensive continuous records for particular localities. Siivonen, as well as earlier workers, has postulated a cycle of tree growth paralleling the population cycle.

In Table 1 we have shown the mean cycle length for each series and also the coefficient of variation ("CV") obtained by expressing the standard deviation of the distribution of cycle lengths as a percentage of the mean cycle length. The coefficient of variation permits us to compare the regularity of the cycles in different series as it measures the degree of scattering of the cycle lengths about their average value. It will be noted from Table 1 that in the tree ring series the mean distance between peaks is almost precisely three years and the coefficient of variation about 40 per cent. The similarity of the two distributions is very interesting because the growth of the western Douglas Fir has been found to be very closely correlated with precipitation (Schulman, 1942), whereas Meyer could find no significant correlation between the hemlock ring widths and rainfall. Of course, in these data some of the peaks are very small,

TABLE 1.—FREQUENCIES OF CYCLE LENGTHS

Cycle length (years)	Number of cycles observed									
	Douglas fir	Hemlock	Lynx Canada	Lynx Finland exc. SW.	Field hare Eng.	Rabbit Eng.	Partridge Eng.	Arctic fox Labrador	Red fox Labrador	Red fox Finland
2	187	33	3	26	13	15	40	18	33	25
3	165	23	3	24	16	21	37	47	31	24
4	98	10	4	26	10	13	21	39	42	18
5	39	8	3	7	3	4	12	19	17	15
6	8	1		4	2	1	6	1	1	6
7	4		3	4	2					5
8	3	1		2	1	3	1			
9			3							
10			1		1	1				
Number of cycles	504	76	23	93	48	58	118	124	124	93
Mean length	3.07	3.01	5.82	3.56	3.60	3.53	3.21	3.50	3.37	3.66
CV (%)	37.3	40.0	50.1	41.5	48.1	47.5	39.8	27.1	31.1	40.0

even representing growth below the average of the entire series. It is very probable that longer cycles are represented in these data but we are not here concerned with this problem.

A further interesting comparison may be made for the years 1703–1939 for which we have data on both tree species. Comparing the peak years we find that for the hemlocks 83 peak years occurred in these 237 years, while for the firs there were 81 peak years. By pure chance, if the series were entirely independent, we should expect to find $\frac{81 \times 83}{237} = 28.37$ as the most probable number of years in which both species would show a growth peak. Actually, just 27 of the peak years coincided—a fact which suggests complete independence of the two series.

For the animal cycles shown in Table 1, with the single exception of the Canada lynx, the mean cycle length is approximately $3\frac{1}{2}$ years and the coefficient of variation ranges from about 30 per cent to about 50 per cent.

HYPOTHETICAL CYCLES

Palmgren (1949) has suggested the possibility of regarding population fluctuations as random variations. The philosophical implication of this suggestion is simply that population increase or decrease is influenced by a multitude of small haphazard factors just as a coin tossed into the air is affected by air currents, variations in the tossing movement, irregularities of the surface on which it lands, and many others so that the result is unpredictable —although we insist that in the last analysis something determines the result. Can it be that populations fluctuate in an analogous manner, *i.e.*, as random variables?

One has only to consult a treatise on ecology (see, for example, Allee *et. al.* 1949, especially Ch. 22) to appreciate the many factors which affect populations in diverse ways. Storms, floods, fires, droughts, sequences of thawing and freezing, extremes of weather conditions, and a variety of other factors are known to affect populations—sometimes only if they happen to occur during some short critical stage of the life history. Can it be that the cumulated effect of so many possible influences, each with a certain probability of occurrence in a particular year, will in the aggregate appear as random population fluctuations?

In order to answer this question we must deduce the results to be expected if populations did fluctuate in a random manner. If this hypothesis predicts a cycle length of about three or four years (as Palmgren found using cards and dice) with not too extreme variations in cycle lengths and gives a distribution of the cycle lengths not too unlike those shown in Table 1, we shall be forced to serious consideration of Palmgren's suggestion. If this simple hypothesis should prove inadequate, perhaps the next simplest hypothesis is that the adequacy or inadequacy of the environment to support population growth varies randomly from year to year. In the following discussion we shall consider the theoretical distribution of cycle length for these two cases: 1) when population size in a given year is a completely random number independent of population size in previous years, and 2) when the quality of the environment fluctuates randomly about its

average value leading to population growth in favorable years and population decrease in unfavorable years.

Various other apparently logical hypotheses could be set up and tested; the writer in fact began his study of this problem by considering more complex cases which seemed biologically realistic,[1] but the lack of refinement in the empirical data forced abandonment of all but the simplest hypotheses. One might, for example, adopt hypotheses of the type a) a population peak is produced when some randomly varying condition of the environment happens to exceed a particular fixed value, or b) population peaks arise when some randomly varying feature of the environment happens to remain above average for a particular number of fixed time intervals. However, in these cases the subject decreases in interest simply because there are too many ways of accounting for the observed facts insofar as their characteristics are definitely known.

DERIVATION OF THE HYPOTHETICAL DISTRIBUTION OF CYCLE LENGTHS

In this section we shall illustrate the means of finding the results to be expected under what seem to be the two simplest situations which might lead one to postulate population cycles. Our general approach follows that given for one of the cases by Kendall (1946, sec. 21.44) in a form which, unfortunately, is not easily followed.

The general procedure is to obtain a distribution giving the relative numbers of "phases" of different lengths, where a phase is simply the distance between two successive turning points, maxima or minima, in the series. It takes two consecutive phases to compose a "cycle"; so, having the distribution of phase lengths, it is easy to combine probabilities and obtain the distribution of cycle lengths. For example, a cycle of two years must consist of a phase of one year in length followed by an opposite phase of one year, a three-year cycle must combine a one-year and a two-year phase in either order, and so on.

For obtaining the distribution of phase lengths we shall call our total number of observations N; find the expected number of phases of any given length, say x years; and by dividing this by the total possible number of phases of all lengths we shall obtain the proportion of the phases which, under our hypotheses, should be expected to be of length x.

CASE 1. COMPLETELY RANDOM FLUCTUATION

Suppose a) that a population is invariable in size but is estimated with varying accuracy from year to year, or b) that the numbers of individuals of a species in a given area are independent of the number present the previous year, being subject only to completely random fluctuations. In either case a graph of the data would show a series of fluctuations with alternating peaks and troughs. Our problem is to determine whether the peaks would be spaced with sufficient regularity to be interpreted as a "cycle." At first glance neither of the schemes which might give rise to this type of series seems biologically realistic. There can be no doubt about the

[1] For biologists wishing to consider other hypotheses, a working knowledge of the elementary theory of recurrent events may be obtained from the book by Feller (1950).

reality of the population fluctuations and certainly the population built up during the reproductive season cannot be completely independent of the initial breeding stock. On the other hand, in the tree-ring data it seems quite possible that the amount of growth made in any one year may be independent of the amount made the previous year. In animals too, when the reproductive potential is exceedingly high and is normally balanced by high juvenile mortality, it is conceivable that factors affecting fertility or juvenile survival might so outweigh relatively large differences in initial breeding stock that a considerable degree of independence might be obtained. This simplest case of completely random fluctuation, then, seems to demand our first consideration.

As a step in deriving the theoretical distributions, let us assume that we have picked a large number, N, of random numbers and have plotted them in order of drawing on graph paper as illustrated in the third line of Figure 1. We shall assume for the moment that the probability of ties, *i.e.*, the chance that any two successive numbers are identical, is so small that it can be neglected. Such a graph then would exhibit a number of turning points, peaks and troughs, with upward and downward phases of various lengths connecting the successive turning points. Consequently, our series will show one fewer phase than turning points and the total possible number of phases will be known when we determine the total possible number of turning points.

We note that in order to have a turning point we must have three consecutive observations so arranged that either the highest or the lowest of the three values is in the center. There are $3! = 6$ [2] equally probable orders in which three random observations might occur and in only four of these will the highest or lowest value be in the middle. Therefore, the probability that any three consecutive random numbers determine a turning point is $4/6 = 2/3$. In our N numbers there are $N-2$ groups of three consecutive numbers and on the average $2/3$ of these groups will determine turning points. Therefore, the average number of turning points in a graph like the one we have been describing is $2/3\ (N-2)$. The number of phases being one less than the number of turning points, we obtain for the average number in such a graph:

$$2/3\ (N-2)-1 = \frac{2N-7.}{3}$$

Now, having the average number of phases for a graph constructed from N random numbers, we have only to multiply this figure by the number of different possible graphs which could be obtained from these numbers in order to get the total possible number of phases. Clearly, the N "observations" could have been drawn in N! different orders so that the total number of phases is:

$$\frac{N!\ (2N-7)}{3} \quad \ldots\ldots\ldots\ (1)\ [3]$$

[2] The symbol n!, called "factorial n", is simply the product of all the numbers from one up to and including n, *i.e.* $n! = 1 \times 2 \times 3 \times 4 \ldots \times n$.

[3] Actually, the correct number is just two greater than given by equation (1) as we are neglecting the two arrangements out of N! in which all of the N points would be in phase with each other. This considerably simplifies the arithmetic and is justifiable for all practical purposes. Even when N is as small as 10, a series far too short to be profitably examined for cycles, the odds against such a single-phase arrangement are more than 1½ million to one.

All that remains in order to determine the distribution of phase lengths is to determine the expected number of phases of length x in a series of N random numbers and then, by assigning different values to x, to build up the distribution.

We note that a phase of length x consists of $x + 1$ consecutive observations all in phase, but in order to know that the phase is of *exactly* length x we also need to know that the first and last points in the phase are turning points. Consequently, we need two additional points or a total of $x + 3$ observations to determine a phase of exactly length x. The expected number of phases of any length x then may be obtained by finding the number of ways of picking $x + 3$ consecutive points and multiplying this number by the probability that any $x + 3$ consecutive points determine a phase of exactly length x.

For any arrangement of N observations it is easily seen that there are $N-x-2$ ways of picking $x + 3$ consecutive observations. Since the N observations could be arranged in N! different ways we obtain $N!(N-x-2)$ as the sought-after total number of ways of selecting $x + 3$ consecutive observations.

Now as for the probability that a particular set of $x + 3$ consecutive observations determines a phase of exactly length x, we note that for any given number of points there are just two ways of arranging them so that they are all in phase. Thus, out of a set of $x + 3$ observations the probability that any particular $x + 1$ consecutive points will be in phase is $\frac{2}{(x+1)!}$; the probability that a particular group of $x + 2$ points will all be in phase is $\frac{2}{(x+2)!}$; and the probability that all $x + 3$ of our points will be in phase is $\frac{2}{(x+3)!}$. For our $x + 3$ points we want the probability that *only* the central $x + 1$ points will be in phase and this will be given by taking the probability that these $x + 1$ points are in phase and subtracting from it the probability of having either of the two possible groups of $x + 2$ points all in phase. Because there are two groups of $x + 2$, the probability of having $x + 2$ in phase is clearly $2\frac{2}{(x+2)!}$. However, all $x + 3$ points might be in phase and the probability of this event, $\frac{2}{(x+3)!}$, must be subtracted from the above in order to obtain the probability of having *just* $x + 2$ observations in phase. Consequently, we obtain: $\frac{4}{(x+2)!} - \frac{2}{(x+3)!} = \frac{4x+10}{(x+3)!}$ as the probability that a phase of $x + 2$ will occur in our $x + 3$ points.

The desired probability that we shall have a phase of $x + 1$ points but none of $x + 2$ is then found to be: $\frac{2}{(x+1)!} - \frac{4x+10}{(x+3)!} = \frac{2(x^2+3x+1)}{(x+3)!}$. Multiplying this expression by the total number of ways of selecting $x + 3$ points we obtain:

$$\text{Expected number of phases of length x} \quad E_x = \frac{2N!(x^2+3x+1)\,(N-x-2)}{(x+3)!}. \quad (2).$$

We have now only to divide equation (2) by equation (1) to obtain the proba-

bility (p_x) of a phase of length x in N observations. This gives us:

$$p_x = \frac{6(x^2+3x+1)\ (N-x-2)}{(x+3)!\ \ (2N-7)} \quad \ldots . \quad (3).$$

Using different values for x, we combine the p_x values to get the frequency distribution of cycle (rather than phase) lengths ("P_x") as follows: $P_2 = p_1p_1$, $P_3 = 2(p_1p_2)$, $P_4 = p_2p_2+2(p_1p_3)$, $P_5 = 2(p_1p_4+p_2p_3)$, and so forth.

This gives us the distribution we have been seeking, the relative numbers of "cycles" of different lengths to be expected in a series of completely random observations. The relative frequencies of different cycle lengths vary slightly with the length of the series, N, long cycles being slightly more probable in very long series of observations. However, this difference is of no practical importance for our present purposes and Table 2 may be taken as adequately representative of the expected distribution of cycle lengths in a series of random numbers.

TABLE 2.—THE EXPECTED DISTRIBUTION OF "CYCLE" LENGTHS IN RANDOMLY FLUCTUATING POPULATIONS

Cycle length "years"	Percentage of cycles
2	39.1
3	34.4
4	17.4
5	6.5
6	1.9
7	.5
8 or more	.2

The mean "cycle" length for this distribution is 3.0 and its coefficient of variation is 37 1/3 per cent. Referring back to Table 1 we note that these values are identical with those obtained from the Douglas Fir tree-ring data, the longest series of biological observations considered. *Most of the "population cycle" data, in fact, show greater variability in the "cycle" lengths than would be expected if the populations fluctuated entirely at random!*

This conclusion seems to the writer to be of considerable interest. The regularity of the population cycles has been considered a feature of economic importance for the purpose of forecasting crop damage and yields of game, and yet we have found no greater regularity than would be expected to result from random variation. If we grant that population cycles are valid for forecasting purposes, then it must follow that a completely random series shows sufficient regularity in the distance between successive peak values to be useful for forecasting, and that the mere act of attributing population oscillations to random fluctuations does not obviate predictions based on fluctuations in population size.

The degree of regularity represented by a coefficient of variation of about 40 per cent may perhaps be better appreciated by reference to an example. Holzinger (1924) studied the visual acuity, as measured by the distance at which type could be read without glasses, of 3373 British males aged 6 years to 80 years and found a coefficient of variation of 39.1 per cent. Many features of our everyday life obviously involve some notion of an average visual acuity, and it seems that population oscillations, or the oscillations in series of random numbers, may be regarded as sufficiently "regular" for some practical purposes.

In Table 3 a comparison is made of the observed distributions of cycle lengths using the more nearly adequate series from Table 1 and the distributions to be expected using the results

TABLE 3.—COMPARISON OF OBSERVED DISTRIBUTIONS OF CYCLE LENGTHS WITH THE DISTRIBUTIONS EXPECTED ON THE HYPOTHESIS OF RANDOM FLUCTUATION

Cycle length	Douglas Fir observed	Douglas Fir expected	Tippett's numbers observed	Tippett's numbers expected	Foxes in Labrador: Arctic observed	Foxes in Labrador: Arctic expected	Foxes in Labrador: Red observed	Partridge England observed	Partridge England expected
2	187	197	110	106	18	48	33	40	46
3	165	173	103	94	47	43	31	37	41
4	98	88	36	47	39	22	42	21	21
5	39	33	19	18	19	8	17	12	8
6	8	9	4	} 7	1	2	1	6	2
7	4	3				} 1			
8	3	1							
Cycle number	504	504	272		124		124	118	
Mean length	3.07		2.92		3.50		3.37	3.21	
CV (%)	37.3		33.2		27.1		31.1	39.8	

shown in Table 2. Also included is a distribution of "cycle" lengths obtained from the first two pages of Tippett's random sampling numbers.

For the tree-ring data, and probably the partridge data, the agreement between observed and expected series seems to be fully as good as in the case of Tippett's numbers. The implication is that these observed three-year cycles are indistinguishable from random fluctuations. Unfortunately, ordinary tests of statistical significance are not valid for testing agreement between observed and expected values in these series. Although there seems to be no completely objective way of deciding, it appears from Table 3 that the data on foxes at least differ somewhat from what one would expect in the case of random fluctuations.

It also seems that the data on animal population fluctuations in general indicate a mean distance between peaks of slightly more than three years rather than precisely three years. There are several ways of explaining this fact without rejecting the hypothesis of complete randomness. One might, for example, postulate that observers have unconsciously been influenced in designating peak years by a preconceived notion that the interval must be some simple fraction of the purported 11-year interval between sunspot minima or that they have been unduly impressed by high peaks and have overlooked some of the small peaks. Probably more pertinent, however, is the effect of tie values in peak years. If the population remains at a peak level for more than one year the result is obviously going to be fewer peaks in any given series of observations and hence an apparent lengthening of the "cycles." Palmgren (1949) has appreciated this possibility and suggested it as an explanation for the fact that observed cycles seem to have mean interval lengths of slightly over three years.

One further point about the oscillations found in series of random numbers requires consideration. Is there any tendency in such a series for every third peak to be a "high peak", a characteristic of animal population oscillations as interpreted by Siivonen? One feels intuitively that if he tabulates only the peaks in a series of random numbers he will still have essentially a table of random numbers and that there will still be a tendency for cycles of length three

to occur. Upon examining Tippett's numbers from this point of view such a "larger cycle" is actually found to occur. If we define a "high peak" as a peak value which is higher than the peak preceding it and the peak following it we find that the mean distance between high peaks is three cycles and the coefficient of variation of this distribution is again about 37 per cent. In terms of actual time intervals this obviously means that a randomly fluctuating population will not only exhibit three-year cycles but also cycles of about nine years between the higher peaks.[4]

Table 4 shows the distribution of cycle lengths between "high peaks" for the Douglas Fir data employed previously and for the first five pages of Tippett's random numbers. In this case also the theory of randomness agrees well with the empirical data and suggests that random fluctuations may explain not only the shorter three to four year population "cycles" but also the longer ten year cycle. It seems almost remarkable that Siivonen's deductions about cycle lengths made from the somewhat inadequate animal population data should accord so well with the behavior of a series of random numbers.

TABLE 4.—THE DISTRIBUTION OF "CYCLE" LENGTHS BETWEEN "HIGH PEAKS".

Cycle length "years"	Tippett's numbers	Douglas Firs
4	10	4
5	24	14
6	28	22
7	30	17
8	25	9
9	30	18
10	15	16
11	11	12
12	9	9
13	3	5
14	13	3
15	8	8
16	9	1
17	1	4
18	3	3
20	2	1
21	—	1
22	—	3
26	—	1
27	—	1
30	—	1
Cycle number	221	153
Mean length	8.97	10.04
CV (%)	39.9	45.8

CASE 2. RANDOM FLUCTUATIONS IN "ENVIRONMENTAL CAPACITY"

The case just discussed based on an assumption of completely random population fluctuations yields oscillatory series with regularity and mean distance between peaks apparently indistinguishable from the short time fluctuations in tree growth. Animal population "cycles" are of a similar type and probably cannot definitely be distinguished from the random case on the basis of existing data. However, many observers have come to the conclusion that the small rodent cycles are four-year rather than three-year cycles. Furthermore, one may question the hypothesis of entirely random variation on biological grounds be-

[4] "High peaks" might also be defined as peaks which are higher than some particular fixed value, or one might speculate that high peaks occur when some random variable in the environment exceeds a particular value. Professor Mark Kac of the Cornell University Department of Mathematics has kindly computed that the mean distance between peaks in such a random series would be given by: $\frac{3}{1-a^3}$ where a is some fixed proportion of the maximum value attainable by the random variable. Employing this interpretation, one can easily propose explanations for "cycles" of any length. For example, if we take $a = 0.89$ meaning that we only count as peaks those years in which our random variable reaches 89 per cent of its maximum value, we shall obtain a mean cycle length of just ten years.

cause of the assumption of independence of population size in consecutive years. This objection is not necessarily vital; Palmgren (1949), in fact, found that a two-point moving average of his random series (a procedure adopted to suggest that both the previous year and the present year influence current population size) gave a series with a mean interval between peaks of about four, and Kendall (1946) has discussed the creation of periodic fluctuations in random data by using moving averages for smoothing. These findings should serve as warnings against the indiscriminate smoothing of population data which has often been employed and they also suggest that some degree of dependence of present population size upon previous size may alter the average distance between peaks even though the controlling factors are random variables. Nevertheless, it has seemed worthwhile to the writer to present the interesting results obtained from one other simple hypothesis of population fluctuations which appears, from the biological standpoint, to rest on very good assumptions.

It is well known that population size is closely related to the favorability or "capacity" of the environment. In a given locality the maximum possible population size for a particular species will be greater in some years than in others and the long-time average number of individuals will be related to the long-time average capacity of the habitat. Now for any particular year we may expect that the odds will be about even as to whether conditions will be above or below average in favorability for population growth. Let us then simply assume that a population tends to grow in every year in which conditions are above average and tends to shrink in every year in which conditions are below average. A population peak then occurs in any year which happens to be above average and also happens to be followed by a below-average year.

The distribution of cycle lengths for this hypothesis may quite easily be worked out by methods exactly analogous to those employed for Case 1, making the substitutions that in Case 2 it requires only two consecutive points (instead of three) to determine a turning point; that a phase of length x consists of just x (instead of x+1) observations; and that only x+2 (instead of x+3) consecutive points are required to determine a phase of length x. This case is analogous to flipping a coin a number of times and counting it as a "peak" whenever a throw of "heads" is followed by a throw of "tails."

If one proceeds to derive the theoretical distribution of phase lengths as we did for Case 1, he obtains for the proportion of the phases which will be of any specified length x:

$$p_x = \frac{N-x-1}{(N-3)\,2^x} \quad \ldots\ldots \quad (4).$$

However, the distribution of "cycle" lengths is given with sufficient accuracy for all practical purposes by the very simple equation:

$$P_x = \frac{x-1}{2^x} \quad \ldots\ldots\ldots \quad (5).$$

Thus, two-year cycles should constitute $\frac{2-1}{2^2} = 1/4$ of the total number of cycles, four-year cycles should constitute $\frac{4-1}{2^4} = 3/16$ of the total number, and so on. In percentage form, the distribution of cycle lengths for this hypothesis is shown in Table 5.

TABLE 5.—THE EXPECTED DISTRIBUTION OF "CYCLE" LENGTHS WHEN THE ADEQUACY OF ENVIRONMENT FLUCTUATES RANDOMLY ABOUT ITS AVERAGE VALUE

Cycle length "years"	Percentage of cycles
2	25.00
3	25.00
4	18.75
5	12.50
6	7.81
7	4.69
8	2.73
9	1.56
10	.88
11	.49
12	.27

The mean cycle length for this distribution is 4.0 and its coefficient of variation is 50 per cent. This hypothesis, then, provides an explanation for the four-year "rodent cycle". It might be noted in this connection that we would expect only about three cycles out of every hundred to be longer than eight years. Since we have no animal population data referring to any number approaching 100 consecutive cycles, it might be expected that very few of the longer cycles would ever have been recorded. Omission of these longer cycles would tend to reduce the coefficient of variation considerably and enhance the appearance of regularity in the oscillations.

Table 6 compares the distributions of cycle lengths in some of the less adequate series of data from Table 1 with the numbers to be expected according to Table 5. Also shown is a long series obtained from Tippett's numbers by reading through the first seven pages and counting a peak as any number larger than 5000 followed by one smaller than 5000. For Tippett's numbers the observed mean cycle length is 3.96 and the table suggests that, although the population data and the theory agree very well with respect to the frequency of the shorter cycles, longer cycles have not been recorded in the animal population data as frequently as this theory would predict.

In the absence of sensitive and accurate tests of the significance of differences between observed and predicted frequencies in distributions of this sort we cannot make a definite statement as to the adequacy of this simple explanation for the four-year cycles. Furthermore, the amount of error made in dating the population peaks in these series is unknown. The theory does predict a cycle length of four years (which would probably be judged as slightly under four years in the short series available to population students) and a degree of variability in cycle lengths comparable to that actually recorded. Under this hypothesis one could

TABLE 6.—COMPARISON OF OBSERVED AND EXPECTED DISTRIBUTIONS OF CYCLE LENGTHS UNDER SECOND HYPOTHESIS

Cycle length "years"	Lynx		Red Fox				Tippett's numbers	
	Finland		Labrador		Finland			
	observed	expected	observed	expected	observed	expected	observed	expected
2	26	23	33	31	25	23	186	178
3	24	23	31	31	24	23	179	178
4	26	17	42	23	18	17	134	134
5	7	12	17	16	15	12	105	89
6	4	7	1	10	6	7	45	56
7	4	4			5	4	29	33
8	2	3					12	19
9							6	11
10							8	6
11		3		14		6	6	3
12							2	2

probably also expect a longer cycle between high peaks, presumably with an average length of about 12 years.

DISCUSSION

A theorist neglecting most biological considerations might expect animal populations to fluctuate in size. Populations of small animals almost necessarily undergo a certain amount of "winter-kill" or its equivalent each year and they typically expand in "summer" through reproduction. It seems obvious that not all years can be regarded as equally favorable in promoting winter survival and summer reproductive success. It is a remarkable property of organic populations that they possess compensatory mechanisms which tend to stabilize population size despite wide fluctuations in the aggregate of forces favoring population change. Briefly stated, these compensatory mechanisms operate in such a way that they tend to maintain the population at a level approaching the capacity of the environment for the type of organisms in question. Compensation is imperfect, however, and we do observe great population fluctuations especially where, as in high latitudes, environmental fluctuations are extreme and in species which possess high reproductive potentials and hence can respond rapidly to environmental changes.

It is to be expected that environmental changes will sometimes be too rapid for the production of parallel population changes. A species in which each female produces only one or two offspring per year will ordinarily have a high rate of survival and cannot be expected greatly to exceed its usual performance in response to one or two favorable years while species with higher reproductive potentials can exhibit dramatic increase in the course of a single exceptional season. Consequently we might expect that more or less fixed life history features such as fecundity, longevity, and age at reproductive maturity would determine a minimum period for any population oscillations. Kendall (1946) has suggested an analogy between many oscillatory systems and an automobile traveling along a bumpy road where the car body describes fairly regular up and down movements with the period determined by inherent properties of the system (load, springs, shock-absorbers) rather than by the disturbing stimuli (bumps) which need not possess any precise regularity. Theoretically then a series of irregularly spaced environmental "bumps" could cause more or less regular oscillations in animal populations, but it is hard to see why so many diverse species should be reported to exhibit the same periodicities under these conditions. For example, there is certainly no reason for expecting chinch bugs (Shelford and Flint, 1943) to possess the same natural period as any mammal. The possibility of population cycles corresponding to some natural population period should not be overlooked and Dymond (1947) in fact seems to suspect the existence of some such phenomenon. However, unless many investigators have misinterpreted population data, it seems very unlikely that we can account for the two predominant periodicities of about three years and of about 10-years on this basis.

The same objections just raised may be applied to the theories which attribute the population cycles to inter-

actions between species. Gause (1935) and DeBach and Smith (1941) have found some experimental support for the theories of Volterra and Lotka which suggest that host-parasite or predator-prey interactions may generate population cycles. However, different two-species systems might be expected to exhibit different periodicities and the evidence (*e.g.*, see Elton, 1942) certainly suggests no consistent interspecies relationships which would account for two predominant periodicities applying to numerous species.

The other general class of explanation for population cycles has assumed a direct determination of population size by some cyclic phenomenon in the environment. Such a controlling factor with a period of forty months could, as Siivonen has postulated, account for both the short cycle and the long cycle. In this case the distance between peaks of the cyclic factor (and populations responding to these peaks) reached in a particular month of the year would be 120 months or 10 years while, in the meantime, other species might respond to each peak regardless of season and hence go through three cycles in 10 years. However, no environmental factor is known to have this period and to be capable of controlling population size.

The conclusions obtained in the present paper suggest that the oscillations of any hypothetical factor determining population size need only be about as regular as would be expected of a random variable. In fact, we need not postulate a particular environmental factor which determines population size. If, as is known to be the case, many factors operate separately to favor population change, we might expect the resultant of the many haphazard influences to appear as essentially random fluctuations. Many variant hypotheses based upon random fluctuations in the environment could be proposed in addition to the two cases considered in this paper and many of them would be adequate to explain all of the definitely established features of the empirical data. Until more precise information is available the preferred hypothesis should be the simplest one consistent with the known facts.

It is, of course, true that in any given situation one or a few particular environmental factors may be expected to exert the predominant influence on population change and it is important to detect such effects. By attributing population cycles to random variation we merely imply that the influence of the environment is not simple. It appears to the writer that the established facts about population cycles do not suggest any mysterious causation and we anticipate that a careful evaluation of the environmental influences on population change will eventually enable us to account for every upward or downward phase observed in natural populations.

LITERATURE CITED

ALLEE, W. C., A. E. EMERSON, O. PARK, T. PARK, and K. P. SCHMIDT. 1949. Principles of animal ecology. Saunders, Philadelphia.

BUDAI, E. 1930. Attraction universelle et énergie vitale. Rev. Path. Comp. et Hyg. Gén. **30**: 712–723.

CLARKE, C. H. D. 1949. Fluctuations in populations. Jour. Mammalogy **30**: 21–25.

CLEMENTS, F. E., and V. E. SHELFORD. 1939. Bio-ecology. Wiley. N. Y.

CROSS, E. C. 1940. Periodic fluctuations in numbers of the red fox in Ontario. Jour. Mammalogy **21**: 294–306.

DEBACH, P., and H. S. SMITH. 1941. Are population oscillations inherent in the host-parasite relation? Ecology **22**: 363–369.

DYMOND, J. C. 1947. Fluctuations in animal populations with special reference to those of Canada. Trans. Roy. Soc. Canada, Third Series, Sec. 5. **41**: 1–34.

EIDMANN, H. 1931. Zur Kenntnis der Periodizitat der Insektenepidemien. Zeits. Angewandte Ent. **18**: 537–567.

ELTON, C. 1924. Periodic fluctuations in the numbers of animals: their causes and effects. Brit. Jour. Exper. Biol. **2**: 119–163.

ELTON, C. 1942. Voles, mice, and lemmings—problems in population dynamics. Oxford U. Press, London.

ELTON, C., and M. NICHOLSON (1942). The ten-year cycle in the numbers of the lynx in Canada. Jour. An. Ecol. **11**: 215–244.

FELLER, W. 1950. An introduction to probability theory and its applications. J. Wiley. N. Y.

GAUSE, G. F. 1935. Experimental demonstration of Volterra's periodic oscillations in the numbers of animals. Jour. Exper. Biol. **12**: 44–48.

GRANGE, W. B. 1949. The way to game abundance. Scribner's. N. Y.

HEWITT, C. G. 1921. The conservation of wild life in Canada. Scribner's. N. Y.

HOLZINGER, K. J. 1924. Note on the relation of vital capacity to certain psychical characters. Biometrika **16**: 139–156.

HUNTINGTON, E. 1945. Mainsprings of civilization. J. Wiley. N. Y.

KENDALL, M. G. 1946. The advanced theory of statistics. Second ed. rev. Griffin. London.

LEOPOLD, A. 1933. Game management. Scribner's. N. Y.

MACFARLANE, R. 1905. Notes on mammals collected and observed in the northern Mackenzie River district, Northwest Territories of Canada. Proc. U. S. Nat. Mus. **28**: 673–764.

MACLULICH, D. A. 1937. Fluctuations in the numbers of the varying hare (*Lepus americanus*). Univ. of Toronto Studies. Biol. Ser. 43.

MEYER, H. A. 1941. Growth fluctuations of virgin hemlock from northern Pennsylvania. Tree-ring Bull. **7**: 20–23.

PALMGREN, P. 1949. Some remarks on the short-term fluctuations in the numbers of northern birds and mammals. Oikos **1**: 114–121.

PETERSON, W. F. 1947. Man, weather, and sun. Thomas Co. Springfield, Ill.

RUSSELL, R. J. 1941. Climatic changes through the ages. In, Climate and Man. U. S. Dept. Agr. Yearbook **1941**: 67–97.

SCHULMAN, E. 1942. Centuries-long tree indices of precipitation in the Southwest. Bull. Am. Meterol. Soc. **23**: 148–161, 204–217.

SCHULMAN, E. 1948. Dendrochronology in northeastern Utah. Tree-ring Bull. **14**: 1–14.

SHELFORD, V. E., and W. P. FLINT. 1943. Populations of the chinch bug in the upper Mississippi valley from 1823 to 1940. Ecology **24**: 435–455.

SIIVONEN, L. 1948. Structure of short-cyclic fluctuations in numbers of mammals and birds in the northern parts of the northern hemisphere. Finnish Foundation for Game Preservation, Papers on Game Research No. 1. Helsinki.

TIPPETT, L. H. C. 1927. Tables of random sampling numbers. Cambridge U. Tracts for computers, No. 15. London.

UICHANCO, L. B. 1936. Secular trends of locust outbreaks in the Philippines and their apparent relation with sunspot cycles. Philippine Ag. **25**: 321–356.

IDENTIFYING PONDWEED SEEDS EATEN BY DUCKS

Alexander C. Martin

Patuxent Research Refuge, Laurel, Maryland

Pondweeds (*Potamogeton*) are probably more valuable to ducks than any other genus of plants in the United States. Their average in the food content of 7,998 ducks from all parts of the country totaled 11%—nearly twice as much as the next highest item, bulrush seeds (Food of game ducks in the United States and Canada. A. C. Martin and F. M. Uhler, U. S. Dept. Agric. Tech. Bul. 634, 1939). Though leaves, stems, winter buds, and other parts of pondweed plants are eaten, the seeds (technically fruits) are particularly relished and found commonly in gizzards.

Most pondweed seeds obtained from duck gizzards can be identified to the species. Small-seeded species, especially when badly worn by digestion, are more difficult than larger ones and not infrequently their determination must be left at "*Potamogeton* sp." Loss of the outer coverings through digestive action often aids identification instead of hindering it. In this respect, wildlife technicians have an advantage over botanists, since the latter seldom have opportunity to see the diagnostic features exposed by removal of the outer tissues.

Additional identifying characteristics inside of the seed can also be capitalized upon if low magnifications of five to twenty powers are available. Lengthwise sections of seeds, parallel to their flattish sides, resemble a human ear (Fig. 1). In these ear-like sections, details of structure vary from species to species—especially in the "comma" and the junction of two tissues in or near its base, the "seam." In making sections for identification purposes, the seed can be held in position by forceps in one hand and cut by a razor blade. Preliminary soaking of seeds in water helps assure good sectioning.

Of the 35 to 40 species of pondweeds in the United States, only slightly more than half are plentiful enough to be significant for waterfowl. Two of the fairly common species, *crispus* and *robbinsii*, are of little value because they are usually sterile. A few species such as

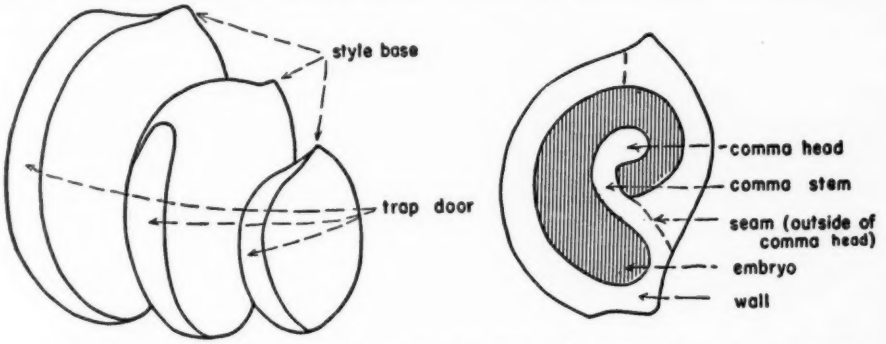

FIG. 1. Seed exteriors and a lengthwise section labelled to identify various parts discussed.

capillaceus, *berchtoldi*, and *oakesianus* have been omitted from the illustrations mainly because their seeds are difficult to distinguish from certain related species. Comments on them, however, are included.

Twenty-one species of pondweeds are illustrated in the three pages of drawings. They are presented in a descending size sequence, from large to medium to small. The figures serve as a sort of pictorial key supplemented by the species comments which follow. All drawings are magnified ten diameters.

SUPPLEMENTAL DESCRIPTION OF SPECIES

Comments on the species are listed in the same order as drawings on the plates. Dimensions cited are only approximate; generally, they represent averages from several specimens.

praelongus; 4− × 3 mm.

Commonly truncate at top; trap door acutely ridged until much worn; comma head occasionally has hole in it; seam in comma is clear and usually another seam extends lengthwise in trap door wall.

zosteriformis; 4− × 3 mm.

Umbilicus variable; though generally distinct, it may wear away with digestion. Seam sometimes not extending into comma head though drawn that way; small hole in comma head gives it appearance of hook as on a knitting needle. Trap door ridged.

amplifolius; 4 × 3½ mm.

Stout style base inclining to persist; trap door prominently ridged; seam distinct and its termination below the comma head definite.

natans; 3½ × 2½ mm.

Unique in comparative narrowness; style base rather persistent; trap door moderately ridged; wall tissues fine and hard. The related species, *oakesianus*, is similar but smaller and has a less conspicuous dimple.

nodosus (*americanus*); 3+ × 2½ mm.

Commonly truncate at top; small end of embryo slightly distended; trap door ridged; seam rather obscure; walls brownish and large-celled.

illinoensis (*lucens*); 3− × 2½ mm.

Similar to *nodosus* but proportionately broader; small end of embryo not distended and more incurved than in *nodosus*.

pectinatus; 3+ × 2½ mm.

Distinct from all other pondweeds (except its close relative, *latifolius*) in its unridged and obtuse apexed trap door. In both *vaginatus* and *alpinus*, the trap door is, as in *pectinatus*, remote from the style base but has an acute apex. Walls thick, large-celled. Small hole may be present by comma or is frequently absent.

vaginatus; 3− × 2 mm.

Similar to *pectinatus* but smaller, more flattened, and top of trap door acute and less remote from style. Low ridge on trap door disappears with wear.

pulcher; 3− × 2+ mm.

Recurvature of comma generally but not always distinct; seed rather strongly angled; trap door ridged.

epihydrus; 3− × 2½ mm. (occasionally smaller)

The only medium-large seed with strongly spiral embryo, slender comma, and thin walls. Trap door ridged and also has low ridges near its margins.

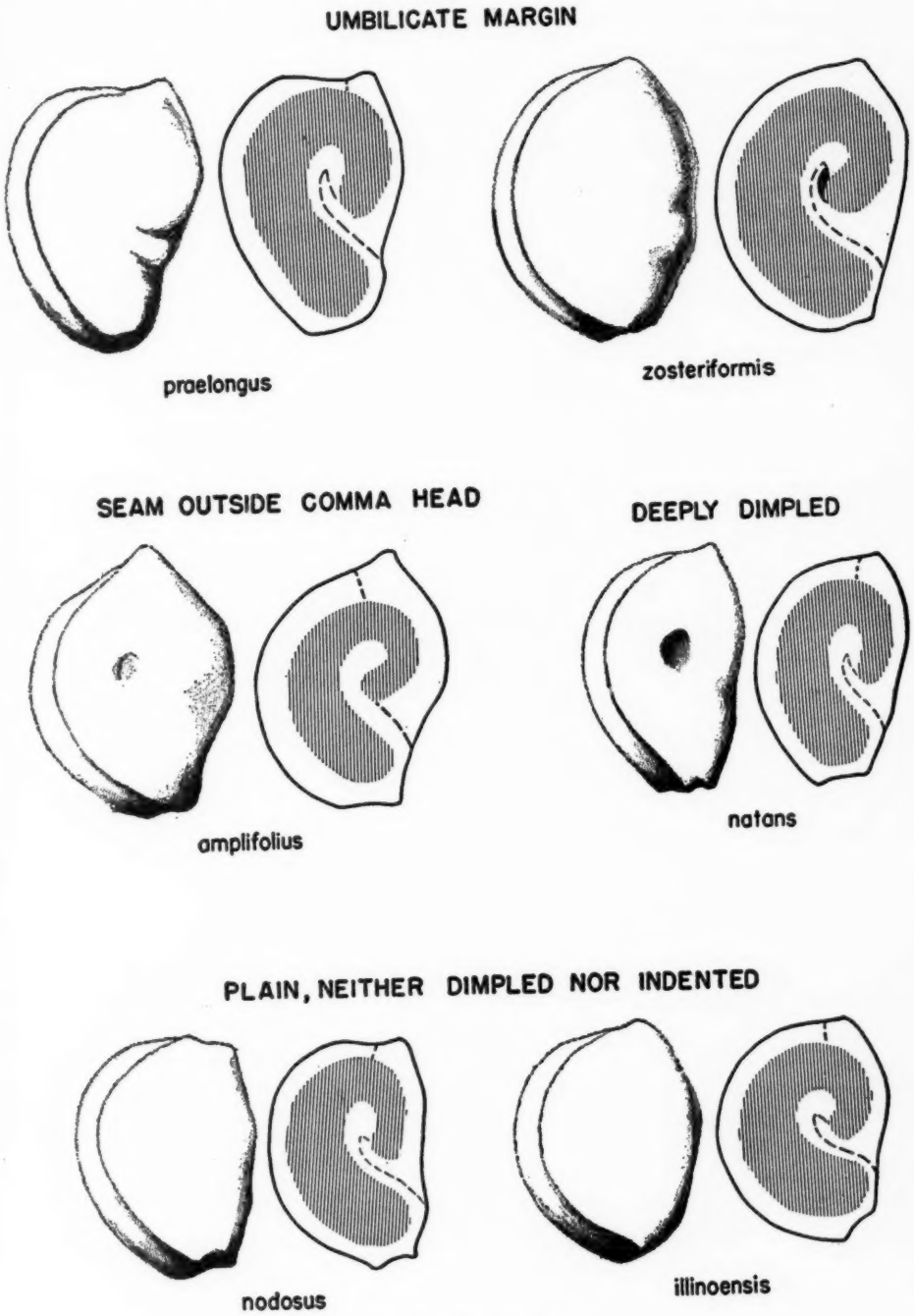

FIG. 2. Larger Seeds.

richardsonii; 3− × 2 mm.

Very thin-walled; flattish; comma head variable in size; sometimes has hole; trap door low-ridged.

filiformis; 2½ × 2 mm.

No ridge on trap door; seam at door top (see drawing) strongly angled; seam by comma a distinct groove.

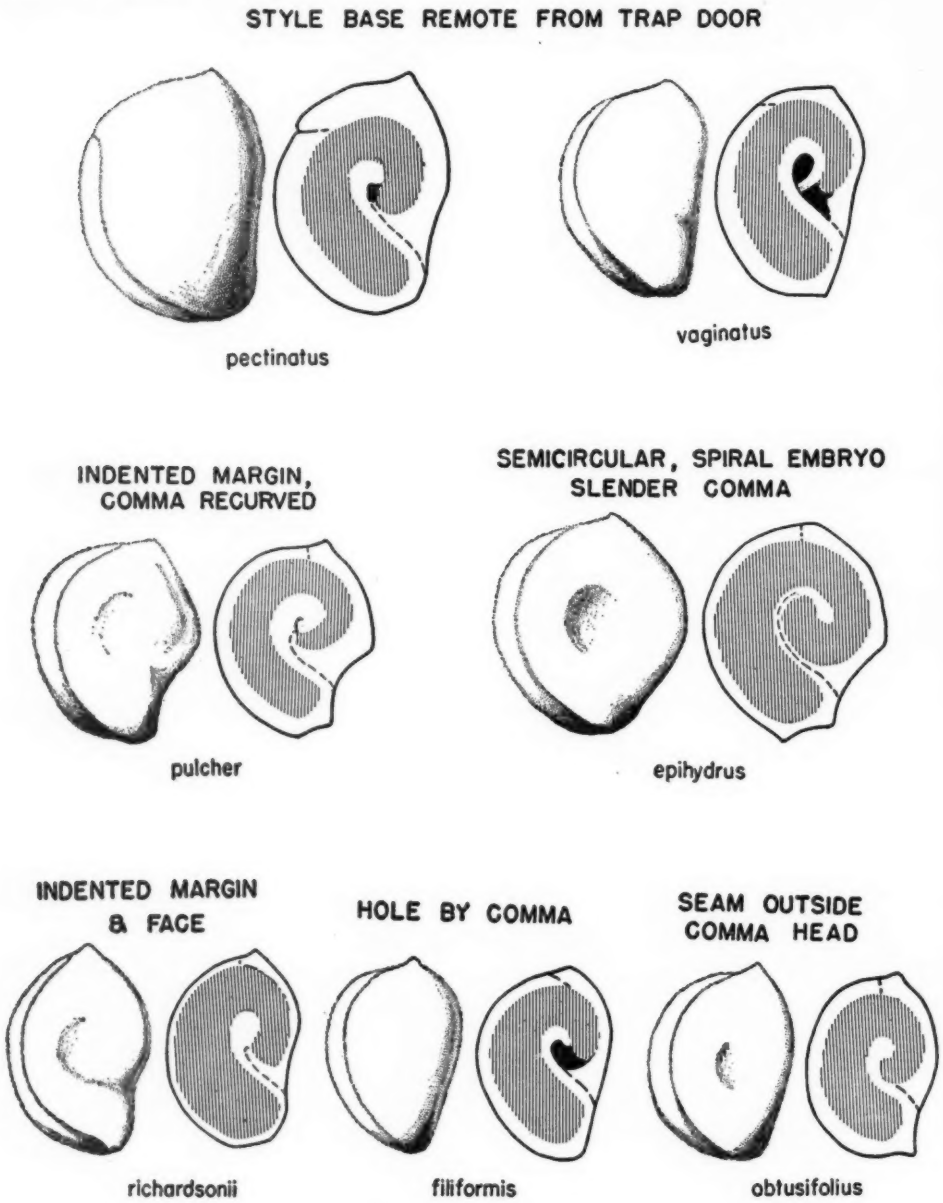

FIG. 3. Medium-sized seeds.

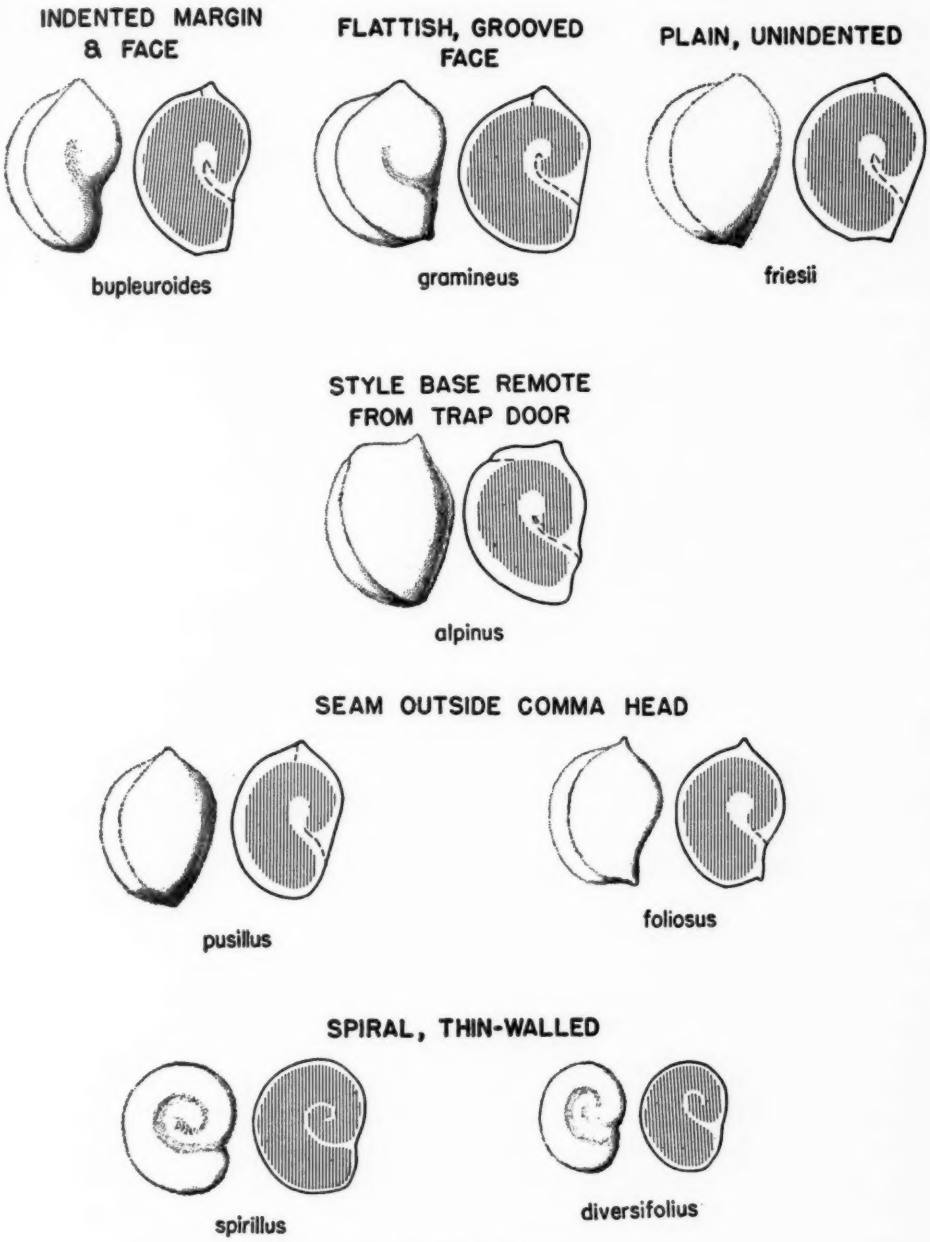

FIG. 4. Smaller seeds.

obtusifolius; 2½ × 2 mm.

Trap door low-ridged; comma stem sometimes more slender than drawn.

bupleuroides; 2+ × 1½ mm.

Like a smaller edition of *richardsonii* with porportionately thicker wall.

gramineus (*heterophyllus*);
2+ × 2− mm.
A low rim is present along trap door margins.

friesii; 2+ × 2− mm.
Comma head large; trap door rounded, inconspicuous.

alpinus; 2½− × 2− mm.
Very distinctive in its truncate, saddle-back top; much like a miniature of *pectinatus* but with acute apex of trap door.

pusillus; 2 × 1½ mm.
Wall proportionately heavy; trap door smoothly rounded and becoming obscure with wear; comma variable, often broader than drawn. The closely-related *berchtoldi* differs from this and the next species in having its seam enter the comma head.

foliosus; 2 × 1½ mm.
Wall thin; ridge on trap door narrow and becoming obscure with wear; points at top and base may also be eliminated by digestive abrasion.

spirillus (*dimorphus*); 1½+ × 1½ mm.
Like the next species below but broader and larger. Wall very thin, comma slender, and embryo strongly spiral.

diversifolius (*hybridus*); 1½ × 1+ mm.
Smaller than *spirillus;* indistinguishable from *capillaceus.*

Accepted for publication November 8, 1950.

THE FOOD OF THE OPOSSUM IN NEW YORK STATE

W. J. Hamilton, Jr.

Department of Conservation, Cornell University, Ithaca, N. Y.

Despite its abundance and wide distribution, the food of the opossum, *Didelphis v. virginiana,* has not been thoroughly investigated. Most food studies have been made from scat analyses or of trapped animals taken in November and December. Audubon and Bachman (1849), Dearborn (1932), and Taube (1947) have made contributions to our knowledge of the food of northern opossums, while Lay (1942) has studied the food of the Texas opossum.

The present study is concerned with the food of the opossum in New York State. More than 200 stomachs have been examined. Of these, 186 have contained food in sufficient quantity to warrant study. Trapped animals which obviously contained nothing other than detritus, fur and flesh from their feet, are not considered in the determinations. The majority of specimens were taken in the central and western part of the state. A few were collected in the southern tier counties that border Pennsylvania. Six specimens were trapped in a bird sanctuary at Quogue, Long Island. One hundred twenty-nine opossums were collected from May through October, while 51 individuals were trapped in raccoon "sets" on the Montezuma marshes of central New York during November and early December. The majority of the specimens were

taken from 1946 to 1950; a few were collected or found on highways from 1932 to 1941.

Stomach analyses give a far better clue to the dietary of a species than do fecal remnants, since the food remains are less digested and thus more readily determined. The almost complete digestion of certain foods before reaching the rectum (earthworms, slugs, etc.) precludes the recognition of these items in the scats. Moreover, scats are not always possible of specific identity unless long field experience with predators has been undertaken. If passed by captive animals, feces are of no worth in natural food analyses unless they are recovered shortly after capture of the animal.

MATERIAL AND METHODS

Large numbers of specimens were secured on the highway. Six dead animals have been observed on a 42-mile stretch of road, three of which were freshly killed and thus suitable for examination. Highway victims may quickly be eviscerated with a single-edged razor blade; the head and reproductive tract may also be removed for study. It is my practice to carry a quart jar of 10 per cent formaldehyde in the car in which the stomach and intestines are placed. Whenever possible, the stomach contents are examined immediately, since the ingested food is easier to determine. Stomach analysis is relatively simple in an animal as large as an opossum. Contents are washed in a large food strainer, a stream of water removing dirt and small debris. The residue is floated in large enamel pans, the various items being separated and computed to frequency and volume. A knowledge of the biota of the habitat frequented by the species under consideration is essential, as is a good reference collection of the larger invertebrates, vertebrates and plants.

ACKNOWLEDGMENTS

I am indebted to members of the New York State Conservation Department for much assistance in securing summer specimens. Dr. W. C. Senning, John Wilson and Lloyd Signs secured the cooperation of several state trappers. Sylvester Vanderbilt supplied specimens from the Montezuma marshes, as did several men trapping the federal refuge on these marshes. Arthur Cooley provided the Quogue animals.

OPOSSUM FOODS

Table 1 includes the analyses of animals taken during the warmer months.

TABLE 1.—STOMACH ANALYSES OF 129 OPOSSUM TAKEN IN NEW YORK, MAY TO OCTOBER. MAJORITY OF SPECIMENS SECURED 1946–1950

FOOD	Per cent of occurrence	Per cent by volume
Insects	59.7	13.4
Fruit	45.7	18.0
Amphibia	36.4	17.2
Mammals	32.6	14.2
Grasses	30.2	6.6
Worms	20.3	5.4
Molluscs	18.6	3.2
Reptiles	19.4	5.3
Birds	13.2	5.0
Carrion	7.0	4.8
Grain	2.3	1.6
Undetermined plants	6.2	1.3
Undetermined animals	6.2	3.4
Centipedes	1.6	.2
Millipedes	.8	trace
Fungi	.8	.2
		99.8

The incidence of food items is reflected in their seasonal occurrence. These items are generally presumed to be taken in proportion to their availability. The opossum does appear to have a spe-

cial predilection for toads, as these amphibians are eaten more frequently than their numbers would indicate.

It is possible that some of the material taken from the stomachs was of secondary origin, i.e., snails, slugs and insects may conceivably have been eaten by amphibians and later exposed in the stomach of the opossum. This possibility is largely precluded in the instance of molluscs, since large numbers were often recovered from the animals when no primary molluscan predator had been eaten. Grasshoppers are usually recovered from stomachs that contain no primary predator.

DISCUSSION OF MAY-OCTOBER FOODS

Insects. When insects are available, they are eaten more frequently than any other food, although the volume may be relatively small. Three-fifths of the stomachs contained insect remains, the incidence of the various orders being in proportion to their size, abundance and ease of capture. Grasshoppers occurred in a third of the stomachs; they were eaten more frequently as the summer waned and these abundant orthopterans increased in size. These insects provide an important food for the opossum well into the winter, when only the dried exoskeletal parts may be found hanging to grass tussocks. Other Orthoptera include numerous crickets, principally *Gryllus* and *Nemobius* and a single mantid, *Stagmomantis*. Lepidopterous larvae occurred in 13.2 per cent of the stomachs. These were principally the larvae of Noctuidae and other small moths, although large saturnid larvae, *Samia* and *Polyphemus*, were eaten. Wooly bear caterpillars, *Isia isabella*, were found in three stomachs. The opossum does not rub the hair from these caterpillars, a trait sometimes credited to the skunk. Beetles were present in 9.7 per cent of the stomachs. Carabids and several large coleopterous larvae were recognized. Tipulid larvae and numerous fly maggots were present in several stomachs. Since maggots were present when no trace of carrion was evident, it is presumed that the opossum may eat the maggots in preference to the carrion on which the fly larvae feed.

Fruit. Occurring in 59 stomachs, fruits constitute an important food of the opossum during and well after the growing season. The animals grub under berry bushes and small shrubs for the fallen fruit, securing these foods long after they have matured and fallen to the ground. In the order of their frequency, grapes, both cultivated and the wild *Vitis vulpina* occurred in 18 stomachs; *Prunus serotina* and *P. virginiana*, 15; raspberries and blackberries, 9; apple, 9; *Cornus amomum*, 3; *Viburnum* sp., 2; tomato, 2; and *Physalis*, 2. Strawberries, blueberries, nightshade, *Solanum dulcamara*, *Rhamnus*, *Amelanchier* and pokeweed were found in single stomachs.

Amphibia. More than a third of the examined specimens had eaten amphibians, a fact quite contrary to the findings of other investigators. These amphibians, in the order of frequency in the stomachs, include the following: *Bufo a. americanus*, 28; *Rana pipiens*, 6; *Rana sylvatica*, 6; *Ambystoma maculatum*, 4; *Plethodon cinereus*, 3; undetermined ranids, 3; and one occurrence each of *Ambystoma jeffersonianum*, *Plethodon glutinosus*, and an undetermined plethodontid. The astonishing large number of toads eaten by the opossum (21.7 per

cent of all stomachs) suggests that this animal may have a pronounced predilection for bufonids. The legs and viscera alone are often recovered from the stomach, although the large parotid glands do not appear to act as a deterrent, for the remains of an entire *Bufo* are frequently encountered in the stomach. A single Florida opossum examined by Pournelle (1950) contained *Bufo* remains.

Mammals. Almost a third of the opossums had eaten mammals. Opossum fur and claws in the stomachs of trapped animals were not considered a natural food, since the animal is known to chew on the foot and leg held by the trap jaws. It is known that opossums will occasionally kill their own kind (Pray, 1921; Seton, 1929). Field mice, *Microtus pennsylvanicus*, occurred in 12 stomachs; *Blarina brevicauda*, 12; *Peromyscus* sp., 6; *Tamias*, 5; *Sylvilagus floridanus mearnsi*, 4; *Sorex cinereus*, 2; *Ondatra*, 2; while each of the following were recovered from single stomachs: *Zapus*, *Condylura* and *Rattus*. Nest young of cottontails, field mice and short-tailed shrews had been eaten. Muskrats were juveniles, as indicated by the small leg bones and metatarsals.

Grasses. Green vegetation, principally grasses and clover, occurred in sufficient quantity in 30 per cent of the stomachs to suggest that this is an important food. The amount in several stomachs indicates grasses are not ingested fortuitously with other food. Some stomachs of animals removed from the highway contained 50 cc. or more of green grasses; this item is not eaten for lack of other foods. Feces do not indicate the green nature of ingested vegetation.

Earthworms. A fifth of all stomachs contained earthworms, principally remains of large *Lumbricus*. Occasionally four or five large nightcrawlers would constitute the greater bulk of the contents. Worms are eaten well into the early winter, long after freezing spells have limited the insect supply. No evidence of this food was found in fecal remains of captive animals that had fed copiously on worms.

Molluscs. Snails were eaten by 16 opossums, slugs by 10. The former included *Triodopsis* and *Anguispira*, while slugs of *Deroceras reticulatum* and *Arion circumscriptus* were eaten in some quantity. One stomach contained 29 *Arion* slugs.

Reptiles. Snakes were found in 25 stomachs. Garter snakes, *Thamnophis s. sirtalis*, occurred in 14 individuals, the redbellied snake, *Storeria occipitomaculata* in 3, and the ring-necked snake, *Diadophis punctatus edwardsi*, milk snake, *Lampropeltis t. triangulum* and DeKay's snake, *Storeria dekayi* each in one stomach. Undetermined snakes (all with keeled scales) were contained in 5 animals. A recently hatched snapping turtle had been eaten by one individual. I have found a small box turtle in an opossum taken on the Vanderbilt Forest at Ashville, North Carolina.

Birds. Eaten by 17 animals, birds make up a prominent part of the dietary. Trace of cock pheasants occurred in 4 stomachs; in three instances these remains consisted of a few breast or flank feathers. It appears probable that the fragmentary remnants do not represent a kill but rather the ingestion of stray feathers. Robins had been eaten 3 times,

crow, meadow-lark, bronzed grackle and vesper sparrow once each, while undetermined birds were found in 7 opossums. These latter are represented by flesh and a few feathers. Quite likely some of the birds were carrion; it appears unlikely that an opossum could capture such a wary bird as the crow. Moreover, the opossum is almost strictly nocturnal and its opportunity for capturing birds must be largely restricted to ground roosting species.

Carrion. Birds, some mammals and other species are killed in large numbers by highway traffic. The opossum probably secures some of its food from this provident table. Items were considered as carrion when maggots accompanied primary food in the viscera.

Grain. Ripening buckwheat had been taken sparingly by 3 animals. The amount of grain eaten by this animal is negligible, in marked contrast to the large amount taken by the raccoon.

Miscellaneous. Undetermined animal and plant material constitute 4.7 per cent of the volume. Fragmentary remains of centipedes and millipedes are presumed to be of primary origin. A single stomach contained fungi which has been tentatively determined as a *Coprinus.*

COMPARISON OF RACCOON AND OPOSSUM FOODS

An excellent opportunity was afforded to contrast the late fall food of the raccoon and opossum on the Montezuma Marsh in 1948. This area has been briefly described elsewhere (Saunders, 1926; Hamilton, 1940). From early November through mid-December trappers take a number of opossums in sets made expressedly for the raccoon. No attempt was made to trap opossums since the pelts at this time (1948) had little value and were scarcely worth the skinning effort. The contrast between the dietary of the two species, collected in an identical habitat, is thus easily made through stomach analyses of the trapped animals.

Trapping was conducted in wet woods bordering large cattail swamps. Many hundreds of ducks and muskrats make their home in this haven. Amphibians, particularly the leopard frog, are extraordinarily abundant. The woods are bordered with arable fields planted to corn, buckwheat, some wheat and oats.

This comparison provides several interesting contrasts and similarities in the dietary of the two species, even though the numbers examined are small. Mammals occur almost twice as frequently in the opossum as in the raccoon. The reverse obtains when insect food is considered. Amphibians were eaten in much the same ratio in both species. Grains provided a substantial part of the raccoon diet at this season; these foods are of minor importance in the dietary of the opossum. Green vegetation, principally grasses, clover and other low growing vegetation are taken in nearly the same proportion by both species. Fruits and berries had been eaten more frequently by the opossum. The much greater preponderance of birds in the opossum food may represent carrion, as mortality among the thousands of waterfowl provide a ready source of food which appears to be eaten more frequently by the opossum than the raccoon. From what we know of the habits of the two animals, the

raccoon would appear to be a more formidable predator of such large prey than would the marsupial. Of the 12 opossum stomachs in which birds were found, 7 contained duck remains; none of the raccoon had eaten ducks.

Incidence of the more important opossum food items listed in Table 2 are:

Mammals: *Microtus*, 16; *Blarina*, 7; *Sylvilagus*, 4; *Condylura*, 2 and *Cryptotis* 1.

Insects: Grasshoppers, 5; beetles, 3; tipulid larvae, 2 and maggots, 2.

Amphibia: *Rana pipiens*, 12; *Bufo americanus*, 1 and *Hyla versicolor*, 1.

Fruits: Grapes (principally *Vitis vulpina*), 9; *Physalis*, 6; one occurrence for *Prunus*, *Solanum dulcamara* and *Cornus paniculata*.

Birds: Ducks, 5; crow, 3; undetermined species, 3; robin and red-winged blackbird once.

Molluscs: *Triodopsis*, 5 and one *Anguispira*.

Reptiles: *Thamnophis sirtalis*, 2; a *Natrix sipedon* and undetermined snake one occurrence.

Winter Food. The severest winter weather may reduce activity of the opossum in central New York, but the animal seldom remains denned for more than a few days at a time. Its fresh tracks attest to activity on nights when temperatures drop to 10° F. or lower. These travels are apparently aimless and usually cover a circuitous route, little more than a mile in a night. The animal usually returns to the original den site. Since a substantial layer of fat is acquired in the fall, the opossum presumably can fare on a minimal food supply during the colder months. Certainly fare is scanty during the late winter. Several times I have followed the winter snow trails of these animals without observing any evidence of feeding activity. Tussocks of grass are explored, particularly on south facing slopes, where grasshoppers seek shelter in the fall. The insect remnants are uncovered by foraging opossums.

Four opossums trapped at Quogue, Long Island, February 10–15, 1950 contained the following items: (1) *Sorex cinereus*, *Microtus* and field corn; (2) *Microtus* remains and fragments of

TABLE 2.—COMPARISON OF NOVEMBER-EARLY DECEMBER, 1948 FOODS OF THE RACCOON AND OPOSSUM ON THE MONTEZUMA MARSH, CENTRAL NEW YORK

	58 Raccoon		51 Opossum	
FOOD	Per cent of occurrence	Per cent by volume	Per cent of occurrence	Per cent by volume
Mammals	32.8	19.0	56.9	30.2
Insects	48.3	9.7	21.6	3.5
Amphibia	29.3	10.4	23.5	8.8
Fruits and Berries	24.1	15.1	37.3	16.7
Grains	36.2	25.3	7.4	4.3
Green Vegetation	27.6	4.2	33.3	5.6
Earthworms	12.1	5.1	25.5	11.2
Reptiles	8.6	4.7	7.9	2.0
Birds	5.2	1.2	23.5	8.3
Molluscs	3.5	1.8	13.5	1.9
Fish	3.5	.8	0	0
Carrion	1.7	2.1	5.9	3.1
Undetermined	6.9	.6	7.4	4.2
		100.0		99.8

green grass; (3) *Microtus*, *Blarina*, grasshopper fragments and mast (probably *Quercus*), green grasses; (4) *Microtus*, *Peromyscus*, carabid beetle fragments, clover leaves. An individual trapped at Quogue, March 30, 1950 had last eaten poultry, a leopard frog, a fringillid and green grasses. Another taken on April 21, 1950 contained poultry feathers, remains of a toad, *Bufo woodhousii fowleri*, *Microtus* and *Sylvilagus* fur.

Every stomach examined had a moderate to heavy infestation of roundworms, *Physaloptera turgida*. More than 200 worms have been removed from a single animal. The infectious stage of this nematode develops in Coleoptera, which constitutes one element in the normal diet of the definitive host. The parasite was determined by Dr. E. W. Price.

SUMMARY

The dietary of the opossum in New York State is reported from 186 stomach examinations, of which 129 were collected from May through October. The principal foods taken in the warmer months, in order of their frequency, are insects, fruits, amphibians, mammals, grasses, earthworms, molluscs, reptiles and birds. Toads occurred in 21.7 per cent of the May-October stomachs.

A comparison of the late fall foods of the opossum and raccoon trapped in the Montezuma marshes of central New York was made. Mammals occur twice as frequently in the opossum as in the raccoon. The reverse obtained with insects. Amphibians occurred slightly more frequently in the raccoon. The opossum eats little grain, an important item in the dietary of the raccoon.

LITERATURE CITED

AUDUBON, JOHN JAMES and JOHN BACHMAN. 1852. The viviparous quadrupeds of North America. Vol. 2, pp. 112–113. New York.

DEARBORN, NED. 1932. Foods of some predatory fur-bearing animals in Michigan. Univ. Michigan School Forestry and Conservation Bull. **1**: 7–52.

HAMILTON, W. J., JR. 1940. The summer food of minks and raccoons on the Montezuma marsh, New York. Journ. Wildl. Mgt. **4**: 80–84.

LAY, D. W. 1942. Ecology of the opossum in eastern Texas. Journ. Mammalogy, **23**: 147–159.

POURNELLE, GEORGE H. 1950. Mammals of a north Florida swamp. Journ. Mammalogy, **31**: 310–319.

PRAY, L. L. 1921. Opossum carries leaves with its tail. Journ. Mammalogy, **2**: 109–110.

SAUNDERS, ARETAS A. 1926. The summer birds of central New York marshes. Roosevelt Wildlife Bul. **3**(3): 335–475.

SETON, ERNEST THOMPSON. 1929. Lives of game animals **4**(2): 892.

TAUBE, CLARENCE M. 1947. Food habits of Michigan opossums. Journ. Wildl. Mgt. **11**: 97–103.

Accepted for publication Dec. 21, 1950.

THE CHUKAR PARTRIDGE IN NEVADA

J. R. Alcorn
University of Kansas Museum of Natural History, Lawrence, Kansas
and
Frank Richardson
University of Nevada, Reno, Nevada

Introduction

The chukar partridge, *Alectoris graeca chukar* (J. E. Gray), a Himalayan subspecies of the red-legged rock partridge occuring over much of southern Eurasia, has been introduced, chiefly in the last two decades, in many parts of the United States. What sucess in establishment it has had, has been chiefly in certain western states (Aldrich, 1947); probably it has been most successful in Nevada. Regular hunting seasons for the species were first possible in Nevada, although they have since (in the autumn of 1949) been opened in the state of Washington. The chukar partridge has, in a relatively short time, built up to rather large populations in some areas of Nevada even though comparatively small numbers of birds were liberated initially. It seems worthwhile, therefore, to record what has been learned concerning the chukar partridge in Nevada, both because continued effort is being made to establish or increase this partridge in this and other states and, more especially, because the population of this partridge has increased to such numbers that a shootable surplus has been produced. It is of considerable moment that another exotic game bird is succeeding in North America as have the Hungarian partridge and the ring-necked pheasant.

Most of the reports on the chukar partridge in different states (Bade in California, 1937; Nagel in Missouri, 1939; *et. al.*) deal mainly with the introduction or artificial breeding of chukars and provide little information on the establishment and consequent activities of the bird in the wild state. Moreland's paper (1950) on the establishment of chukar partridge in Washington, and Nagel's (1945) paper are notable exceptions. The present report is concerned with the establishment of the partridge in Nevada and with understanding its success there. Methods of liberation, activities immediately subsequent to liberation, and establishment under conditions of partial domestication have been studied in Nevada as they were in California (Richardson, 1941) but, except for the last subject, will not be discussed here.

The authors are grateful to G. Hammond Hansen, formerly District Agent in Nevada, of the United States Fish and Wildlife Service, for enabling us to to do much of the field work connected with this study. The field observations of Vernon L. Mills have been valuable. Harold Peer has contributed much information that he gained during the operation of a game bird farm near Stillwater, Nevada and while trapping wild chukars for the Nevada Fish and Game Commission. The facilities of the Department of Biology of the University

of Nevada and funds granted from the University of Nevada Research Fund have been helpful in providing for the analysis of food habit materials and in the preparation of this paper.

LIBERATION

Ira Hamlin Kent of Fallon, Nevada, so far as known, was the first person to bring chukar partridge into Nevada. In March, 1933, he received six pairs of the birds from Calcutta, India. He sold birds first in November of 1934. Chukars were planted in Nevada as early as November, 1934, when 50 birds were liberated on the R. L. Douglass Ranch located eight miles south-southwest of Fallon in Churchill County. Many agencies have released chukar partridge since that date. Each of Nevada's seventeen counties managed their own game, and many of these counties released chukar partridge. Local hunting clubs, too, introduced chukars and other species of game birds even previous to 1934. In most instances there are no accurate records of such releases, but it is known that by 1941 all counties, except possibly Mineral and Ormsby, had received chukar partridge.

Figure 1 summarizes the liberation data for Nevada. Although the exact localities where many of the birds were released are unknown, the counties to which the birds were delivered are an indication of the approximate places of plantings throughout the state. In the last three years (1947–1949), many wild birds were trapped at certain places and released in new locations. Such plantings are not indicated on the map except in the relatively few instances where chukars were released in areas well away from their previously established range.

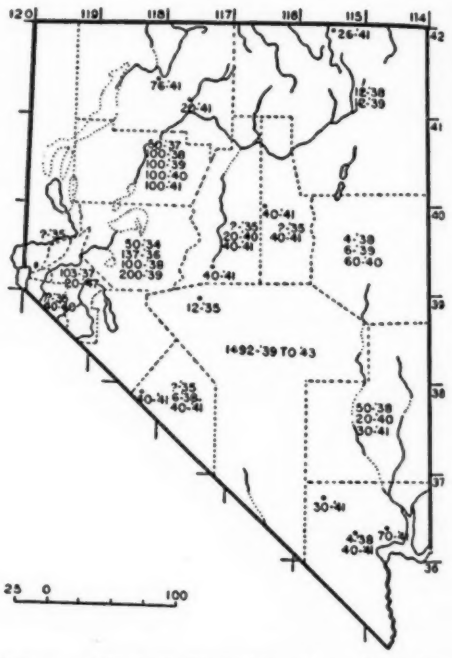

FIG. 1. Principal liberation sites of chukar partridge in Nevada. Numbers of chukar partridge and years when liberations made are placed in the centers of the counties when localities of liberations are not known. Dots indicate approximate localities of known liberations.

Consideration of the extent and numbers of plantings shown in Figure 1 suggests successful establishment in many parts of the state. County associations or sportsmen's groups, with limited funds, repeatedly purchased numbers of partridge as the success of the birds became known. The total number of chukars that have been liberated will never be known, but our records show that it would run to between five and ten thousand individuals. The birds have increased so greatly in numbers, and have already been planted so widely, that relatively few additional liberations of pen-raised birds should be necessary. The present

program of shifting birds trapped in the wild to new ranges should be an effective substitute. In the period 1947 through 1949, 1561 birds were thus trapped in Nye County and released in new areas thought to be uninhabited by the species.

PRESENT STATUS

Figure 2 shows the approximate range in which the chukar partridge is now well established in Nevada. In general, the bird has bred and become permanently established in mountain ranges of approximately one-half of the state, and occurs to a lesser extent in lower country, including the broad Lahontan Valley. The greatest centers of population seem to be in the central part of the state between latitudes 38° and 40° N and longitudes 116° and 119° W. These concentrations can be correlated, in part, with more numerous liberations in earlier years in these areas, but it seems also that the habitat is most suitable for the partridge in these areas.

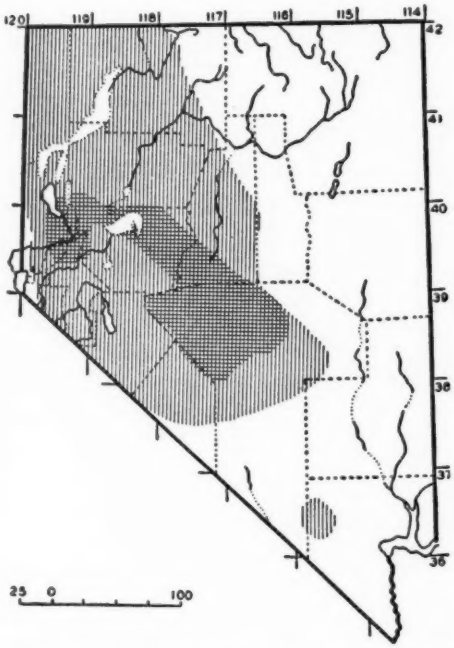

FIG. 2. Distribution of established populations of chukar partridge in Nevada. The area of cross hatching indicates the area where these birds are most abundant.

During October of 1948, 1949 and 1950, the Nevada Fish and Game Commission declared an open season of from two to fourteen days on chukar partridge in more than half of the state. This is an indication of the large populations of the birds most of which, because of the places in which they live, are not readily accessible to hunters. Hunting was limited to residents (hence probably approximately 10,000 partridge hunters), the bag limit was set at five birds a day or in possession, and the shortest season was set in the region of the densest human population. Figures are not available on the actual take of chukars by hunters. Probably the figure would be low per hunter because many of the gunners were unsuccessful. However, it was estimated that more than 1,000 birds were taken by hunters in the 1949 season from a single canyon area not exceeding one-half mile wide by four miles long. Another estimate was of 2,000 chukars taken in Nye County in the 1949 season.

Hunters generally are not yet well acquainted with the chukar partridge and its habitat and habits. Even when found, a covey often eludes a hunter by flying across a sizable canyon or by running or climbing up a rocky mountainside. In spite of the rather rigorous hunting demand, the chukar is already a popular game bird in the state among many of the sportsmen.

ESTABLISHMENT

Many of the early liberations of partridge were in valleys devoted to agriculture and supplemental feeding induced these birds to stay near farms. Mrs. Anna Bailey Mills has had chukars breed with some success for the past thirteen years on her farm four miles west-southwest of Fallon and in fields adjacent to it. In the first year she used Bantam hens to incubate the partridge eggs and to raise the young. More young were raised by this method the next year and approximately two-thirds of the young were allowed to run free. These birds did not leave the area but returned to roost near the captive partridge. In the next year, more young were raised by the bantam hens, but the liberated chukars also made nests and reared young. All chukars, except the original pair, were then liberated, but few, if any, left the Mills' ranch. In the summers of 1939–1940–1941 many of the birds nested on the Mills' Ranch and other pairs were noted on surrounding ranches as far as three miles away. In the fall of 1941, there were approximately 100 partridge roosting on the buildings, haystack, and ground of this ranch. Small flocks occurred on many surrounding ranches. Each year these birds were fed in winter. Predators were kept down in the area. Only part of the birds were liberated each season. Chukars were fed until accustomed to foraging for themselves. In the change from captivity to liberation there was little or no sudden change in feed, water, or environment. We hesitate to say whether the birds at this locality will continue to hold their own. In November of 1949 approximately 20 birds remained on the Mills Ranch. The earlier (1941) concentration of 100 birds at this roosting site is thought to have been an over-population and the resulting pressure probably caused the birds to scatter to surrounding farmlands.

The above described establishment of chukars through semi-domestication has probably not been tried with equal thoroughness or comparable success in other parts of Nevada. Studies by one of us (Richardson) of chukars liberated in Southern California, indicated that the species may become weakly established under partial domestication but that the desired breeding and extension of range is most likely to occur in steep, rocky, open (in regard to vegetation) and relatively arid country away from habitations and agriculture. There is much territory of this kind in Nevada. (Plate 10) Under proper conditions, the liberation of the birds in wild country where there is at least a temporary supply of food (with access to agricultural areas in some cases) seems most likely to be successful.

Many partridge that were planted in the lowlands of Nevada drifted to the surrounding hills and mountains and there became well established. Chukars seem to be found most typically in canyons, especially those with at least small amounts of water. In many instances pen-raised birds were never seen again in the locality of their release. Often they again appeared in late fall at ranches ten or more miles away. Farmers have frequently fed them in the cold, snowy, winter months. Under these conditions chukar partridge at times reverse their usual tendency to move into and stay in mountainous country.

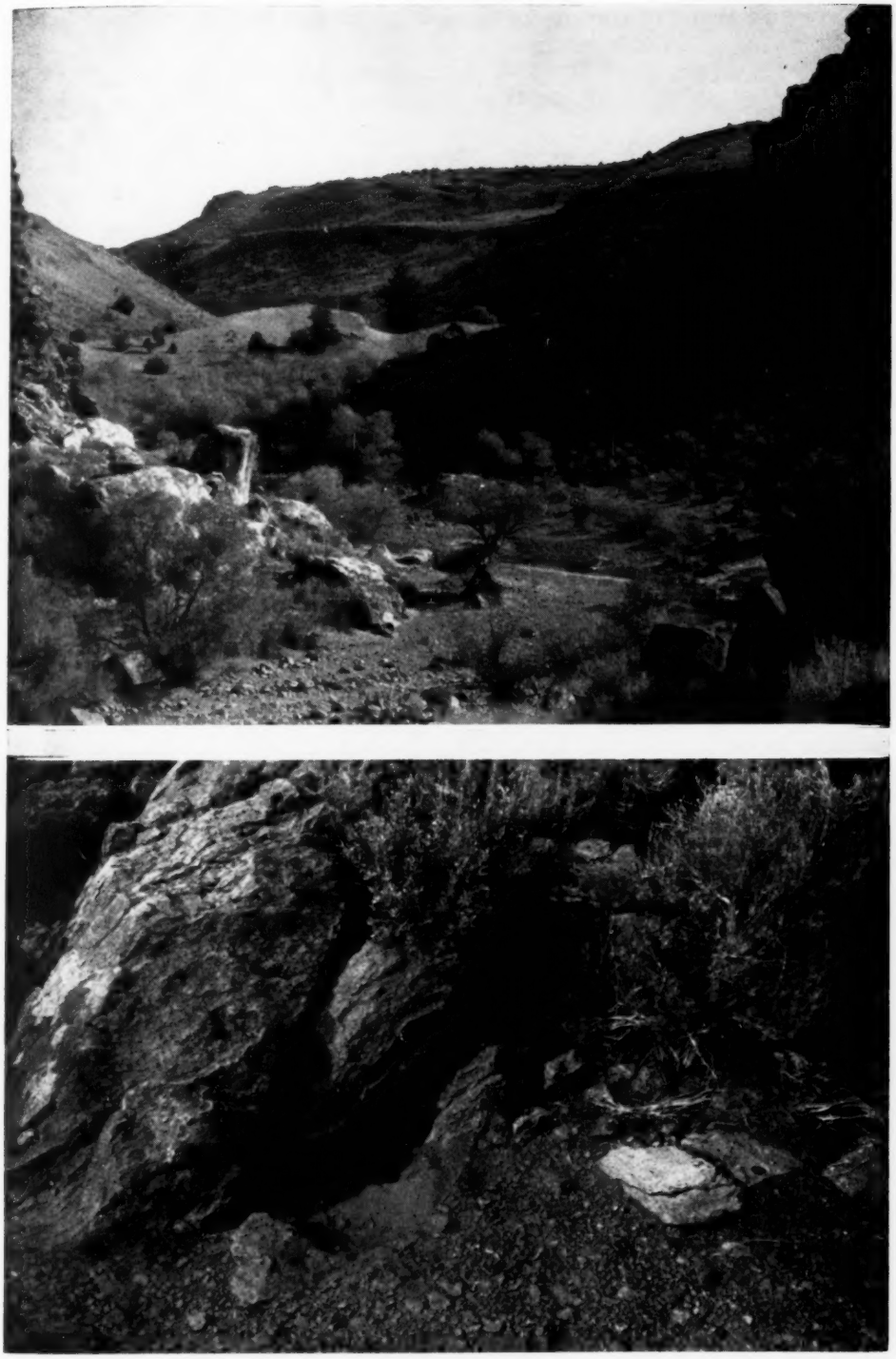

PLATE 10. (Upper) Typical chukar partridge habitat in the Virginia Mountains, Washoe County, Nevada. There is permanent water and a growth of willows in this canyon. (Photograph by Frank Richardson). (Lower) Dusting spot of chukar partridge. The most used dusting space was approximately 8 inches across. (Photograph by T. C. Franz.)

The earliest date, of which we have record, when chukars certainly were established in Nevada was 1939 in Storey County, but the birds may have been established there and/or in other parts of the state a year or two earlier. In a region where liberated, chukar partridge may not be seen until several years later when they have greatly increased their numbers and then, rather suddenly, are seen frequently. Harold Peer believes that the birds actually change their habits, becoming markedly bolder when their population has built up to a certain density.

LIFE HISTORY

The following natural history account of the chukar partridge is based on birds which had been established for several years and unless otherwise indicated had been living quite independently of any human assistance. Further work with such well established birds will be of great value because it is in the rocky, relatively barren areas making up many thousands of square miles of the western United States, that the chukar may become an important game bird.

Feeding Habits—Although the crops of only 41 chukar partridges have been analyzed (Tables 1 and 2) and although field observations on food habits are not extensive, it is already indicated that chukars are adapting themselves to a wide variety of available foods. It is of particular interest that chukars are utilizing abundant foods, such as the blades and seeds of introduced grasses, which appear to be used relatively little by other birds. These foods and others such as filaree seeds and pine seeds, are being utilized extensively in Nevada without competition with other game-birds. Moreland (1950) shows that where chukars have been notably successful in Washington, they also are depending on abundant foods including introduced grasses.

Thirty-four of the 41 crops of wild birds were taken in late October and November, whereas only one or two were taken in each of the months of July, August and September. A larger number of crops, more widely distributed over the year, would be desirable. Nevertheless, certain major conclusions can be drawn from a study of the available material. As shown in Table 1, downy brome (*Bromus tectorum*, bronco or cheat grass) seeds, and grass blades, probably usually downy brome, form an outstanding item of diet. Seeds and blades of *Bromus rubens* (a closely related species, also from the Mediterranean) constitute part of the food but are sometimes not distinguishable from downy brome in crop contents. Certainly both of these grasses, but especially *Bromus tectorum*, are present in abundance over most of the chukar range and often are the dominant plants in burned over or over-grazed areas. The basal shoots of native bunch grasses (probably usually *Poa secunda*), and probably the green blades of these perennial grasses, form extremely important food. It is particularly significant that several kinds of grasses are available at all times of the year; grass blades usually are present from early November through the winter and spring (and in the summer near water), and the seeds are present all the year but are most readily available in summer and fall.

Field observations and inspection of

TABLE 1.—ANALYSIS OF CONTENTS OF CROPS OF 41 CHUKAR PARTRIDGE, FROM WILD HABITAT IN CENTRAL AND WEST-CENTRAL NEVADA, COLLECTED IN LATE SUMMER AND EARLY AUTUMN OF 1941, 1948 AND 1949

SEEDS—	*Number of Crops Containing*	*Comments*
Plant Foods (More than 98% of all crop-contents)		
Bromus tectorum (Downy Brome)	23	Over 80% of contents of 12. (Over 900 seeds in 1.)
Erodium cicutarium (Filaree)	11	95, 60, 45, 20, 15, and 5% of contents in 6. (Over 2,000 seeds in 1.)
Pinus monophylla (Pinon Pine)	5	Almost entire contents of the 5. (32 pine nuts in 1.)
Helianthus sp. (Sunflower)	6	A few in each.
Lomatium sp. (Desert Parsley)	3	35, 20, and 5% of contents.
Polygonum sp. (Smartweed)	3	40% of contents of 3.
Arabis sp. (Rock-cress)	3	30 and 20% of contents of 2.
Descurainia pinnata (Tansy-mustard)	3	80 and 5% of contents of 2.
Grayia spinosa (Spiny Hopsage)	3	5% of contents of 2.
Astragalus sp. (Loco-weed)	3	A few in each.
Amsinckia sp. (Fiddleneck)	3	A few in each
Argemone hispida (Prickly Poppy)	2	5 and 4% of contents.
Lupinus sp. (Lupine)	2	A few in each.
Oryzopsis hymenoides (Sand Bunch-grass)	1	4 seeds.
LEAVES—		
Poa secunda (a Blue-grass)	14	Over 98% of contents of 3. Over 40% of 5 others.
Green grass blades	22	Over 95% of contents of 3. Fair amounts in 7 others.
Green dicot leaves	9	40% of contents of 3.
FRUITS, ETC.—		
Rosa sp. (Wild Rose)	1	5 "rose apples".
Allium sp. (Wild Onion)	1	95% of contents. 19 onions.
Artemisia galls (Sagebrush)	4	20 and 15% of contents of 2. (19 galls, ave. 8 mm. long, in 1.)
Unidentified composite buds		
Unidentified flowers	1	10 flowers.
Animal Foods (None in 31 crops)		
Locustidae (Grasshoppers) (*Melanoplus*)	9	15 grasshoppers in all, 7 in one crop.
Lygaedae (Chinch Bugs and Allies)	1	3 common milkweed bugs.
Formicidae (Ants)	3	8 ants in all.
Stenopelmatidae (Jerusalem Cricket)	1	1 Jerusalem cricket.
Centipede	1	1 large leg.
Small rodent feces	4	207 in all, 50% of contents of 1 (175 feces 4–6 mm. long)

feces have repeatedly indicated the importance to chukars of grass seeds and blades. The droppings of the birds are green in winter and spring when grass blades and filaree leaves (and probably other herbaceous foods especially after late winter) seem to be the chief foods. The digging out of *Poa* and other shoots with the bill has been observed many times. The contents of five crops, as well as field observations, show that pine seeds (*Pinus monophylla*) are a major food at some times and places. One hunter reported that chukars eating pine-nuts were very fat, and as much as one-half pound heavier than those birds obtained in other localities, whose crops contained green grass and seeds of bronco grass. Piñon pines are numerous in almost all of the mountain ranges in which chukar partridge occur. Although pine

nuts may be excellent food for chukars, the nuts are not an entirely dependable food because the crop of nuts in a given area may be good one year and poor the next. Many pine seeds are hollow but partridge seem to have excellent judgment in selecting sound nuts, for of the 63 examined in crops none was hollow. Chukars have been seen to climb into rabbit brush (*Chrysothamnus*) where they seemed to be eating the flowers or buds. Lupine leaves (probably *Lupinus caudatus*) are a favorite food of chukars in the Stillwater area. The seeds of blazing star (*Mentzelia laevicaulis*) and of wild sunflowers are known to be eaten not infrequently. Wild onions (*Allium*) are common over much of that part of Nevada where chukars occur and are a favorite food at times. The onion bulbs usually lie about two inches below the surface of the ground; consequently much digging, done with the bill, is required. The flesh is less palatable after the birds have eaten many onions than at other times.

Animal food seems to be of secondary importance, but a knowledge of the food of young chukar partridge in the wild may show that animal food is important to them. Adults eat only occasional insects or other animal foods, with some preference for grasshoppers. The latter were found in small numbers in several crops from widely separated places.

The crops listed in Table 2 are too few to provide trustworthy answers to several questions but make it clear that young birds eat large numbers of insects in agricultural areas. The young birds whose crops are listed in Table 2 were taken in an alfalfa field, a habitat where a great variety of insects would be expected. The great predominance of potentially detrimental insects in the chukar crops is a point in favor of the partridge in agricultural areas.

Field observations add to the list of

TABLE 2.—ANALYSIS OF CONTENTS OF CROPS OF 8 CHUKAR PARTRIDGE, FROM AGRICULTURAL HABITAT IN THE LAHONTAN VALLEY, CHURCHILL CO., NEVADA, COLLECTED IN THE EARLY SUMMER OF 1942. SIX CROPS OF YOUNG BIRDS

	Number of Crops Containing	*Comments*
Plant Foods (More than 60% of all crop-contents)		
Triticum Seeds (Wheat)	2	Over 85% of contents.
Populus fremontii Seeds (Cottonwood)	6	Over 50% of contents of each of 6 young birds.
Green grass blades	1	9% of contents.
Elaeagnus angustifolia Fruits (Russian Olive)	1	4 fruits (c. 15mm.)
Animal Foods (About 45% of contents of 6 young)		
Aphididae (Plant Lice)	6	40, 35, 33, 28, 27, and 24 specimens.
Lepidopterous larvae (Butterflies and Moths)	6	28, 20, 19, 17, 15, and 14 specimens.
Cicadellidae (Leafhoppers)	6	23, 18, 13, 12, 9, and 8 specimens.
Miridae (Plant Bugs)	6	19, 7, 4, and 2 specimens of *Lygus pratensis*. 4 and 1 others.
Corizidae (Corizid Bugs)	3	4, 3, and 2 specimens.
Locustidae (Grasshoppers)	3	2, 2, and 1 specimens.
Lygaedae (Chinch Bugs and Allies)	2	3 and 2 specimens of *Geocoris*.
Araneida (Spiders)	3	3, 2, and 1 specimens.
Occasional items: Carabidae (Ground Beetles), Curculionidae (Alfalfa Weevil), Blattidae (Roaches), Pentatomidae (Stink Bugs), Formicidae (Ants), several unidentified Coleoptera, Heteroptera, Diptera and Hymenoptera.		

foods in cultivated regions. Corn and wheat, especially during the winter and spring, are known to be well-liked foods. Shoots are dug out with the bill, then eaten. Serious damage to recently sprouted fields of corn has been reported when chukars dug out the kernels. Chukars have been observed climbing up the sides of corn shocks to reach ears. Several tons of potatoes were destroyed by the digging and feeding of chukars in one locality. Although the partridge apparently do not especially like fruit, they have been observed to eat raspberries, currants, strawberries, and apples. Fruits of Russian olive are eaten perhaps rather frequently, but chukars have not been observed climbing into the trees for them. Instead, they pick up the fruits from the ground, or, where there are low hanging limbs with fruit, jump more than a foot into the air to seize fruits with their bills.

Chukars feed almost entirely on the ground or on vegetation that they can reach when they are standing on the ground. Only occasionally will a bird jump into higher vegetation. However, several people have reported seeing them in the tops of piñon trees, seemingly obtaining pine seeds. On the ground, the bill, rather than the feet, is used almost exclusively in obtaining food. It is not infrequently used in forceful digging as for basal shoots of grass and for wild onions.

Relation to water—Water is scarce in most of the chukar partridge range in Nevada, and is in widely scattered small springs and occasional creeks. In summer, chukars tend to be limited to the areas where water occurs. At no time, in summer, were partridge found far from water. In the summer of 1941, a person could find from one to six coveys in the vicinity of several springs in Storey County. At a spring in the Virginia Mountains approximately five coveys, comprising some 100 birds, came to water daily. Flocks usually flew to the spring in early morning, drank, and sometimes spent several hours near the spring before leaving. At no time at this place were birds found at the spring in late afternoon. Several groups of partridge living in the vicinity of a small but permanent creek (shown in Plate 10) that flows through the Virginia Mountains for several miles seemed to have similar drinking habits. The birds were frequently observed to drink from the overflow on the ground below the stock water troughs and never were seen to drink from the troughs although they were low enough for domestic sheep to use. In winter and spring the birds are widespread. In these seasons snow or rain and succulent green plants probably afford adequate moisture; also the cool weather in these seasons may lessen the birds' need for water.

Cover and Roosting Habits—Although the habitat of chukars almost always affords fair brushy cover, the birds depend on it little. Chukars being approached by a person almost invariably run up the hill or canyon side, climbing with speed and agility over rocks, rather than seeking cover. Numerous droppings and signs under juniper and piñon trees indicate that chukars may spend much time there. These trees probably afford protection from heat and possibly from enemies. Wild rose bushes were seen to serve as cover near an exposed mountain spring, and the north side of smaller bushes served as shelter from the sun.

Chukar partridge seem to prefer to

roost among rocks and in open areas, even though trees may be available. This preference was observed in the Virginia Mountains and also with newly liberated chukars on the Mojave Desert. Steep, rocky slopes were selected for roosting in both places. In the Lahontan Valley, the birds roosted both on the ground and on buildings.

Breeding habits—Pairing takes place in the spring. We have little data on the time of pairing but probably it usually takes place by the middle of March. Nagel (1945) reports pairing of chukars in February and March in Missouri. Our observations and other reports both indicate that monogamy is of a brief type. Males tend to band together while the females are nesting. When the young are half-grown or larger, the males return and flocks of both sexes and several ages result.

Nesting may start in early May, although more often later, and may continue through July. Most of our nesting records are from the Lahontan Valley; consequently we cannot accurately state the time of nesting of chukars in the mountainous areas. The number of eggs observed in nests (of at least 14 nests seen) varied from 8 to 16. The average was near 9 eggs. Possibly some of the largest clutches were laid by two females. One nest found in Lahontan Valley (May 25, 1942) contained nine chukar eggs, six ring-necked pheasant eggs and one valley quail egg. In another nest in this valley, nine chukar and six valley quail eggs were successfully hatched. On another occasion, a female chukar partridge successfully hatched a chicken egg. Mr. Peer says that of the numerous young seen by him, 12 birds was the general rule with a high of 14 and a low of 8. All of these were in mountainous areas. From the southern end of the Toiyabe Range in Nye County in 1948, he reports abandoned chukar nests and others with only one to three eggs. There was some indication that these birds (and also quail, doves, and sparrows in the area) were diseased. The number of young seen in the Lahontan Valley with parents or other adults, in seven instances, averaged only 7.5. Somewhat in contrast to Nevadan records, Nagel (1945) found 2 to 19 eggs in Missouri chukar nests and 13 pairs produced an average of 14.6 young.

Nesting sites in Lahontan Valley, according to Mrs. Mills, were perhaps most often on high ground such as on the brush-covered and weed-covered hillocks interspersed in the cultivated areas. Five nests found in alfalfa fields in 1941 were destroyed by mowing. A good many nests were found in a small orchard but many of the clutches there failed to hatch. One nest was in an open area of wire grass and another, unsuccessful, in a clover pasture. The behavior of nesting females, when molested, was to lower and vibrate the wings while issuing distress cries. One female did not leave her eggs until her tail feathers were pulled. Then she ran from the nest feigning injury and turned from side to side. Another female with young did not fly until touched. According to Vernon Mills, the males take no part in rearing the young and have been seen roosting together in the nesting season.

Dusting—Dusting is a well-developed habit in chukar partridge. Plate 10 shows a typical dusting spot. It was photographed in March on the south side of a rock on a fairly high ridge.

Chukars seem to enjoy the dusting and sprawling on a dry sunny slope in winter. In summer they frequent shady places and use cool, damp soil for dusting. On one occasion, approximately 50 young and adult chukars were seen in a clearing near Fallon and many of them were dusting themselves in the dry, dusty soil. All the birds appeared to be moving considerably. One that was walking suddenly squatted and scratched dirt into its feathers, and then continued walking. This action was typical of others that dusted themselves. On other occasions, chukars dusted themselves for half an hour at a time.

Notes or Calls—Chukar partridge do much "talking" among themselves. Their low chucking sounds are often frequent when they are feeding or when an adult female is moving about with her young. When alarmed into flight in the daytime a characteristic cry which can be heard at some distance is almost invariably given. When alarmed into flight at night, however, they have never been heard to give this cry. Their loud chucking call seems to serve as a means of locating one another especially when a covey has been scattered. This call is a series of notes often given at fairly regular intervals of approximately 25 seconds. Each series lasts from four to as many as eight seconds. The call usually starts with a few weak but clear notes followed by loud "chucks", and is frequently given from a vantage point such as the top of a rock. On several occasions when chukars became aware that one of us was in a blind, they stretched their necks and looked at the blind while issuing short rasping notes. One observation was that birds were silent when feeding, whereas, in the same flock, those that were not feeding were noisy with their "chucking" notes.

Predators—Little has been learned of predation on chukar partridge in Nevada. On two occasions golden eagles were seen to circle or perch near partridge. Cooper hawks were twice seen under circumstances which indicated that the hawks were hunting chukars but so far as known the hawks had no success. A red-tailed hawk swooped on a partridge which had been liberated (in a mountain range of the Mojave Desert in California) less than an hour before. The hawk then followed the chukar as it flew across a canyon, but made no further attempt to catch it. In this same area, another red-tailed hawk was seen to flush two chukars. Apparently there was an attempt to capture one for the hawk remained on the ground momentarily.

The reaction of chukars to hawks is to remain quiet and hidden. The partridge were absolutely quiet for a full 20 minutes after the first of the above mentioned red-tailed hawks had departed, and were nearly quiet for almost an hour more. Similar reactions were noted after the appearance of the Cooper hawks and eagles mentioned above. The chukars remained silent for 17 minutes in one case—some 12 minutes after the hawk had disappeared. Mrs. Mills states that the partridge on her ranch start for cover when danger threatens, or squat on the ground if cover is not close by.

Harold Peer once observed a coyote surrounded by chukars. The chukars were talkative and approached close to the coyote, but when the coyote trotted in the direction of a chukar, the bird

ran to one side. Mr. Peer's presence frightened the coyote away.

G. H. Hansen reports that in May, 1941, an adult chukar partridge, seemingly greatly disturbed, was seen on top of a fence post. Examination revealed a gopher snake at the bird's nest. The snake had swallowed six of the eight eggs. The snake was killed and five of the eggs were replaced in the nest. The partridge then continued to lay in the nest until it contained 11 eggs, all of which later hatched. It may be noted, too, that snakes, especially yellow-bellied racers, were the worst predators on eggs and young of chukars being raised four miles west of Fallon by Alcorn in 1937.

Seasonal Movements — Chukar partridge tend to seek the higher parts of their range in summer and the lower parts in winter. This has been clear especially where snows cover the higher range. However, in some places chukars come down to lower ground in the summer or fall seeking water or food in the valleys. In some places in the winter the partridge may, instead of seeking lower areas, take advantage of wind-swept higher areas not apt to become snow-covered. In many areas water may be available only at lower elevations in the summer or fall, and in these places chukars tend to have little vertical migration. The notes on establishment have already indicated that chukar partridge tend to leave the broader valleys or return to them only at times in the winter.

Summary — Between five thousand and ten thousand chukar partridge have been liberated in Nevada beginning in 1934. In recent years a transplanting program within the state has been undertaken. The species now is sufficiently abundant, chiefly in rocky, mountain ranges of the central and western parts of the state, that, in 1948, 1949, and 1950, an open season was allowed in approximately half of the state. The length of the season varied from two to 31 days with a possession limit of five birds. The crops of young birds contain many insects. The chief foods of adults appear to be: *Bromus tectorum* seeds; filaree seeds; pine seeds; basal shoots of native bunch grasses; and green grass blades. At all seasons the birds roost among rocks or in open areas instead of in or among trees. In summer the birds are restricted to areas with water. In winter, chukars move to higher areas from which the snow has been blown away and some move to lower areas if more food is available there. Males may band together in the nesting season. The average number of eggs in 14 nests was nine.

LITERATURE CITED

ALDRICH, JOHN W. 1947. The Hungarian and chukar partridges in America. U. S. Dept. Int., Wildlife Leaflet **292**: 1–10.

BADE, AUGUST. 1937. The chukar partridge in California. Trans. N. A. Wildlife Conf. **2**: 485–489.

MORELAND, RALEIGH. 1950. Success of chukar partridge in the state of Washington. Trans. N. A. Wildlife Conf. **15**: 399–407.

NAGEL, WERNER O. 1939. A preliminary report of the chukar partridge study in Missouri. Trans. N. A. Wildlife Conf. **4**: 416–421.

———. 1945. Adaptability of the chukar partridge to Missouri conditions. Jour. Wildl. Mgt. **9**(3): 207–216.

RICHARDSON, FRANK. 1941. Suggestions for future liberations of chukar partridges in Southern California. California Div. of Fish and Game (Unpublished report).

Accepted for publication Oct. 27, 1950.

AN AGE DETERMINATION METHOD FOR THE PINE MARTEN [1]

William H. Marshall [2, 3]

Division of Entomology and Economic Zoology, University of Minnesota, St. Paul, Minn.

During field and laboratory studies on western pine marten (*Martes caurina caurina*) in central Idaho the desirability of developing a ready method of age determination for these valuable fur animals became apparent and considerable effort was expended to obtain and study carcasses of wild-caught marten for this and other purposes. One hundred and seven complete carcasses were received from trappers in that area during the winters of 1937–38, 1938–39, 1939–40. The following proposed method of age determination was developed from data on these carcasses, as well as on five skulls and bacula loaned by Phillip Wright of Montana State University, two skulls of known old age from the Saratoga Springs Experimental Fur Farm, and five skulls of very young marten from the same source. More recently Dr. Wright has made available for study ninety-two skulls, weights of twenty-six bacula, and data on occurrence of *corpora lutea* of seventeen female marten from northwestern Montana.

To be of maximum value an age determination method should be of the type that is readily observable, easily recorded, and sensitive to advances in age of the animal studied. The method presented here permits determination of breeding and non-breeding classes but may in the future be refined into a more inclusive method for older age classes.

At the time the study was initiated no materials of known age were available. All measurements that were thought to be significant for age determination were taken from the carcasses. These measurements were as follows:

1. Total length, hind foot length, and tail length.
2. Tooth development and wear.
3. Fusing of sutures.
4. Other skull measurements.
5. Weight of bacula.
6. Occurrence of *corpora lutea*.
7. Character of sagittal crest.

The data obtained from these measurements and other records were analyzed with respect to their frequency distribution of occurrence and correlated whenever possible to indicate probable usefulness and/or integration with the available background information from the literature.

[1] Most of the material here presented was submitted to the University of Michigan in partial fulfillment of the requirements of the Doctor of Philosophy degree in 1942.

[2] The author was employed by the Division of Wildlife Research, U. S. Fish and Wildlife Service, while carrying on the field and laboratory work here reported. Sincere appreciation is expressed to Dr. H. H. T. Jackson for his guidance and to the late Professor H. M. Wight for helpful suggestions. Drs. S. A. Graham, Phillip S. Wright and Robert K. Enders assisted directly with the manuscript and materials.

[3] Paper No. 2614 of the Scientific Journal Series, Minnesota Agricultural Experiment Station, St. Paul 1, Minnesota.

The patterns of curves of frequency distribution may be used as indices of progressively changing variation within a population as against a static condition. A normal curve indicates that the character studied is uniform for the particular series being analyzed, whereas a bi- or poly-modal curve indicates variations within the sample which allow for separation into groups on the basis of the factor which may be progressively developing with passage of time. Such curves of frequency occurrence have commonly been used in fisheries studies (Peterson 1895 and Van Oosten 1929) and in white-tailed deer studies (Cahalane 1932) to indicate different age classes where growth continues throughout the life of the individual.

The possibility that such variations may be racial or subspecific in character seems invalid in this study, as some measurements varied in a normal manner while others exhibited bi-modal curves. The original materials all came from a relatively small area, whereas the supplementary material from Montana was more varied in origin. In both cases the data were so similar in distribution that is was considered feasible to combine them. Dr. Wright states that characters he has used for racial determinations of the pine marten have been the length of the auditory bulla and the anterior-posterior length of the lingual moiety of the upper molar.

The available data on marten, with a discussion in each case of their significance and relation to other data, are presented below.

1. *Total length, hind foot length, and tail length.* Data from the 107 carcasses from Idaho showed a normal curve of distribution in each sex; hence they were not considered to indicate progressive changes in the sample. These data were not available from Dr. Wright's Montana specimens.

Brassard and Bernard (1939:15) investigated the growth of young American marten (*Martes a. americana* Turtox) at the Quebec Zoological Gardens and found that "the adult weight is attained at about three months." Mallner (1931) states that Solowjew worked with young sables in Kamchatka Peninsula that were full grown in July, while Kozantschikov (1930) reports that young sables become full grown and independent during July and August in the Saijan Mountains of Siberia.

2. *Tooth development or wear.* Differences that were amenable to quantitative measurement or analyses in either tooth development or wear were not found in any of the materials.

Brassard and Bernard (*op. cit.*) reported that "adult dentition has appeared at about three months," thus obviating the possibility of using rate of development of dentition as a criterion.

Popov (1943), in discussing use of tooth wear as an age index in Mustelinae, states: "It presents, however, considerable difficulties, and the reliability of the results obtained is largely dependent on the expertness and skill of the worker. This is because the degree of attrition is determined not from mathematical quantities, but is expressed by relative notations, such as slightly worn, medium attrition, strongly worn."

3. *Fusing of sutures.* The presence of unclosed sutures in the mammalian

skull is generally regarded as an indication of immaturity in the animal. However, the type of treatment which the skull has received during preparation may influence this character. Three sutures—the fronto-parietal, basi-sphenoidal, and nasal—were examined in detail.

No instances of unfused fronto-parietal or basi-sphenoidal sutures were found. This is in agreement with Grinnell et al (1937:188) who state of the western marten: "In both sexes the sutures between the various bones become quickly closed." Deansely (1935: 467) found in the stoat of England that "these sutures close and disappear fairly early in development."

On the other hand, in twenty-four of eighty-one male skulls and thirty-five of one hundred and seven female skulls unfused nasal sutures were found. Here, then, was a character of the skulls which differentiates two groups and which may indicate age differences.

4. *Other skull measurements.* The width of the post-orbital constriction and of the zygomatic breadth was measured with a micrometer caliper. These data showed poorly defined bi-modal curves which may have indicated two age groups, i.e., immature and mature. This is in agreement with Grinnell et al (*op. cit.*) who indicate that these measurements are difficult to make and require careful cleaning of the skull, so they are considered of minor importance.

Data on width of rostrum, width of post-orbital processes, and height of brain case fell into a normal curve pattern, so were not considered further.

5. *Length, diameter, and weight of bacula.* Fifty-eight bacula were obtained from wild caught marten. After being thoroughly cleaned and air-dried the total length and greatest diameter were measured with a vernier caliper and weights were obtained with an analytical balance.

The two measurements of length and diameter were abandoned as possible age criteria. This was because there were a great many variations in the curvature of the bone, while the base of the baculum was often so irregular that diameters could not be accurately recorded.

Data of weights arrange themselves in a bi-modal curve with the peaks near the extremes (Fig. 1) and it was thought this might well indicate immature and adult animals.

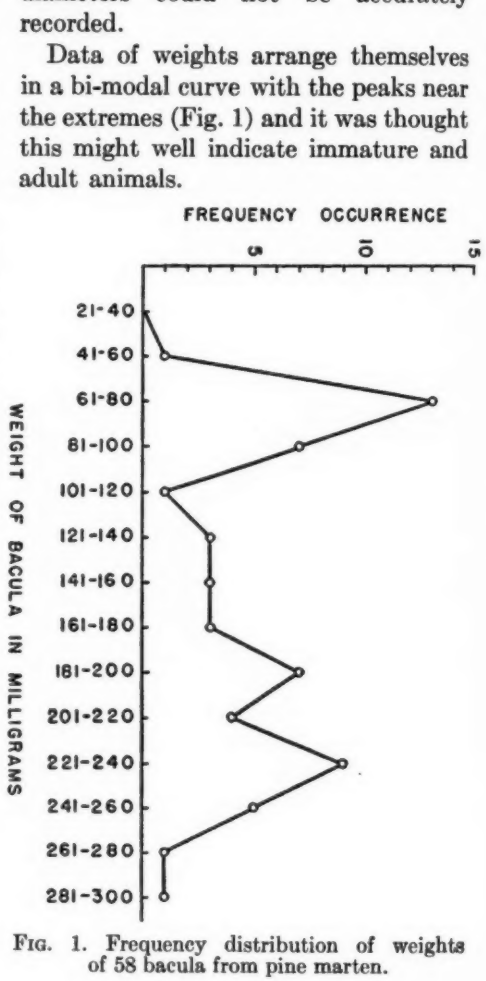

FIG. 1. Frequency distribution of weights of 58 bacula from pine marten.

Checking back to the information obtained from the nasal sutures it was found that all of the animals with bacula weighing over 100 mg. showed fused nasal sutures. On the other hand, only four of twenty-one animals with bacula weighing under 100 mg. had fused sutures. Thus there is a strong tendency for unfused nasal sutures to be associated with bacula less than 100 mg. in weight, i.e., the left hand extreme of the bimodal curve of bacula weights.

Deansely (*op. cit.*) found that in the stoat the baculum "increases rapidly in weight at sexual maturity and shows no definite regression during anoestrous." Wright (1947:344) considers that bacula weights in the long-tailed weasel (*Mustela frenata*) were "of two types with respect to weight . . . . . and appearance." Further: "All animals known to be juveniles exhibited the small type baculum while all animals in active spermatogenesis or known to have been in active spermatogensis . . . . . exhibited the large, mature type baculum." Thus "young males can be distinguished from the adults until . . . . . they are about 11 months old."

Popov (*op. cit.*) reporting on six members of the Mustelinae states ". . . . . within each species . . . . . these small bones fall into two sharply differentiated groups. The first, having no thickened base at the point of fixation of the cavernous body to the baculum, is typical of young animals, while the second group, characterized by thickened well formed base of the os penis is typical of adult individuals." He indicates that a weight of 200 mg. marks the division between adult and juvenile of *Martes m. ruthena* Ogn.

Apparently, then, weight of the bacula may be used to indicate two broad age groups. In field management the collection of such data and/or specimens might be difficult to arrange with trappers for two reasons. First, the nature of the material would be subject to ridicule and secondly, many trappers do not know of, or would miss, the relatively small bones. Furthermore such data are applicable only to males.

6. *The occurrence of corpora lutea.* The ovaries of females collected in Idaho were studied in cooperation with Dr. Robert K. Enders, Swarthmore College, Pennsylvania (Marshall and Enders, 1942), while Dr. Wright supplied data on the Montana specimens. Forty-five ovaries were in sufficiently good condition to be stained and sectioned. No *corpora lutea* were found in twenty of these and inactive endometrial tissue lining the uterine horns indicated that the animals had not bred. *Corpora lutea* were present in twenty-two. One ovary had non-lutienized *corpora lutea.* These animals were thus known to have bred during the previous summer (Enders 1939). Three Idaho specimens had ovaries that had apparently broken down owing to old age and inactive endometrial cells. They were therefore considered to have been too old to breed.

In comparing these data with the occurrence of fused or unfused nasal sutures, it was found that of the fifteen specimens in which unfused nasal sutures were found and for which ovaries were available, none had *corpora lutea.* Of the twenty-eight with fused nasal sutures and for which both types of data were available, four had no *corpora lutea,* twenty-one had *corpora lutea,* and three

had not bred, apparently because of old age. This was considered to be substantial agreement between these two types of data.

Schmidt (1934) points out that captive female sable and spruce marten do not breed during the first summer of their existence, some fifteen per cent mate during the second summer, and all breed during or after the third summer. Thus the presence or absence of *corpora lutea* may be correlated with age groups.

As with the bacula such data would be difficult if not impossible to use under field conditions. Determination of presence or absence of *corpora lutea* involves not only dissection but also refined techniques of fixation and sectioning.

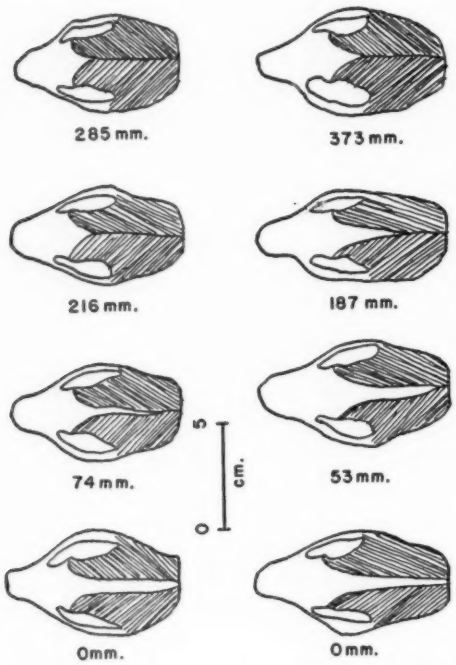

FIG. 2. Semi-diagrammatic outlines of marten skulls as arranged in Plate 11, with length of sagittal crest indicated for each skull. Female skulls on left, male skulls on right.

7. *Character of sagittal crest.* Examination of the material at hand showed striking and obvious differences in the sagittal crests of the skulls. (Fig. 2 and Plate 11).

The sagittal crest is developed at the origin of the temporal muscles. In many skulls there is no "crest" but rather a broad band bordered by two faint lines known as temporal crests. These are found on the parietals and indicate where the temporal muscles originate on these two bones. With other skulls there is a distinct ridgelike "crest" known as the sagittal crest (found along the suture of parietals) that splits anteriorly to form an open "V" which extends to the post-orbital processes on either side of the skull.

The type of variations indicated that the origin of the two temporal muscles moves medially up the parietals to join and form the sagittal crest, which then continues to increase anteriorly in length and also in height by a process of bone deposition. In this way the sagittal crest is first formed and then increases in size with age. Fig. 2 and Plate 11 indicate these variations as they were found in skulls selected to show all gradations.

This concept of changes in the character and length of the sagittal crest as the animals grow older was developed directly from Deansley's discussion of age determination in the stoat as follows (p. 467): "In June . . . . . in both sexes the temporal muscles extend part way up the parietals, and bone, which does not give rise to muscle, is clearly distinguishable in the dry skull . . . . . in the skulls of both sexes collected in July, the temporal ridges, beginning posteriorly at the exterior

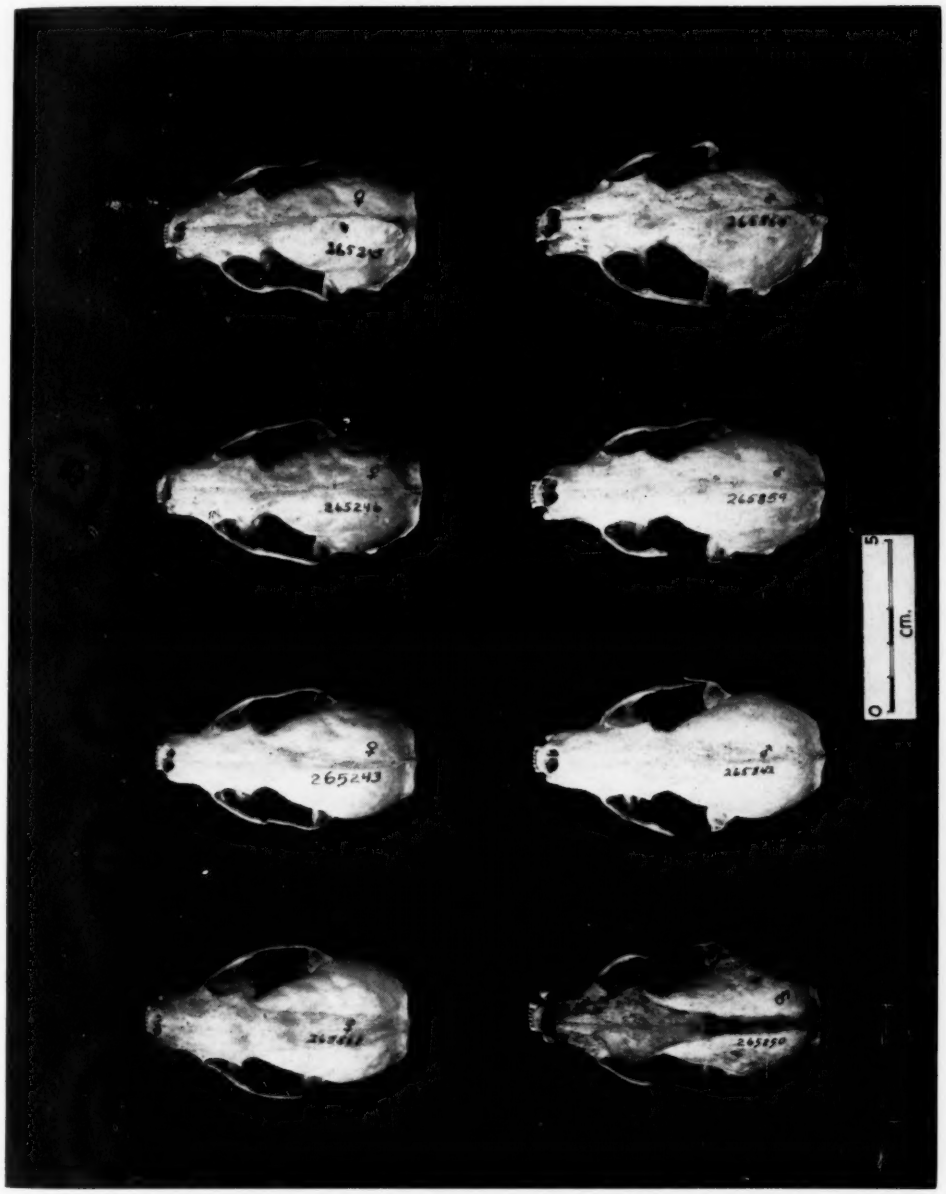

PLATE 11. Dorsal view of series of western pine marten skulls (*Martes c. caurina* Merriam). The age varies from top to bottom with females on left, males on right.

occi
form
form
betv
Dea
cres
her
inte
O
mat
and
poss
D
not
dors
had
meas
from
of th
to
point
parti
musc
crest

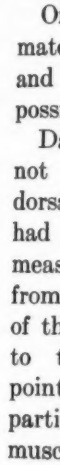

FIG. 3.

occipital protuberance, are fusing to form a sagittal crest; this leads to the formation of a deep "V" open anteriorly between the temporal crests. . . . . ." Deansley apparently found sagittal crests in all stoats by October, though her remarks are rather difficult to interpret on this matter.

On the basis of these assumptions the material at hand was examined in detail and comparisons made with the other possible indices previously discussed.

Data were recorded as to whether or not the temporal muscles had met dorsally to form a sagittal crest. If they had met the length of the crest was measured with a micrometer caliper from its posterior point to the apex of the "V" that bifurcates anteriorally to the post-orbital processes. This point can usually be easily determined—particularily in uncleaned skulls as the muscle is dark red-brown, whereas the crest is whitish-yellow.

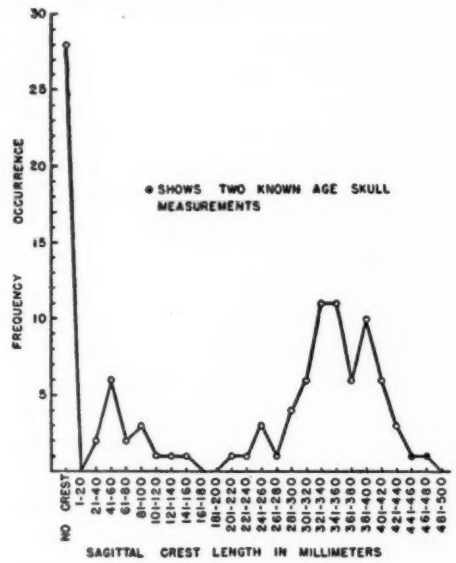

FIG. 3. Frequency distribution of sagittal crest lengths of 110 male marten skulls.

In the case of skulls of male marten (Fig. 3), when a frequency curve of sagittal crest lengths was plotted, a distinct bi-modal curve was found.

In a comparison of data on sagittal crest and nasal suture seventy-two of the seventy-eight skulls with sagittal crests displayed fused nasal sutures. Conversely, of thirty-two skulls with no sagittal crest, twenty-seven had open nasal sutures.

In fifty-eight male specimens the three characters could be compared simultaneously as follows:

Sagittal Crest Length	Bacula	Nasal Sutures	# Skulls
None	Under 100 mg.	Open	13
Under 10.0 mm	Under 100 mg.	Open	4
Under 10.0 mm	Under 100 mg.	Fused	2
10.0–19.9 mm	Under 100 mg.	Fused	1
20.0–29.9 mm	Under 100 mg.	Fused	1
20.0–29.9 mm	100–199 mg.	Fused	5
Over 30.0 mm	100–199 mg.	Fused	10
Over 30.0 mm	Over 200	Fused	22
			58

Here we find a large group at each end of the scale presenting the three characters which indicate either immaturity or maturity. The central group appears to be less well defined.

On the basis of inspection it would seem logical to conclude that male animals with no sagittal crest, or with one less than 20.0 mm. in length, are immature animals, and those possessing crests of larger size are mature animals.

In the female marten (Fig. 4) the curve shows a similar bi-modal character, although much compressed. Thus there is a high frequency of females without sagittal crests and another peak, between 4.0 and 8.0 mm., lengths, whereas in the males the second peak occurred at 30.0 to 40.0 mm. It may well be that the sagittal crest is of a some-

what secondary sexual character in female animals, as Grinnell et al (*op. cit.*) indicate on the basis of nine skulls (p. 187): "Females apparently never get well marked sagittal ridges."

Of forty skulls without a sagittal crest thirty-three had unfused nasal sutures and seven were fused. Of the fifty with sagittal crests, all but one had fused nasal sutures.

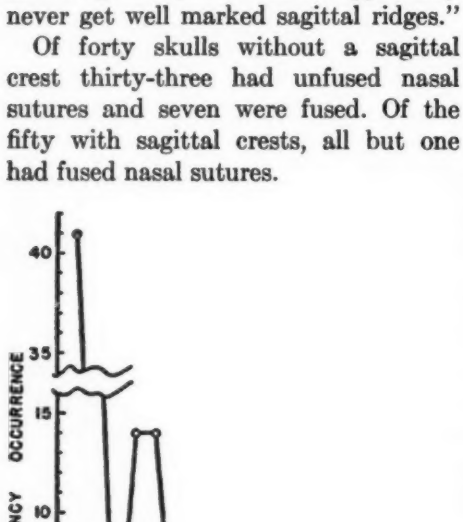

FIG. 4. Frequency distribution of sagittal crest lengths of 87 female marten skulls.

A comparison with respect to presence or absence of *corpora lutea* with that of the sagittal crest in forty-three animals showed that one of the nineteen animals known not to have bred had a sagittal crest, while all but one of the twenty-four animals known to have bred had sagittal crests. Further, the three thought to have been too old to breed showed the greatest length of sagittal crest.

A logical deduction from this inspection and Schmidt's data on breeding age would seem to be that all females lacking the sagittal crest were immature while all with sagittal crests were adult.

These assumptions based on this use of the sagittal crest were checked against the following material of known age. Two skulls of male marten from the Saratoga Springs Experimental Fur farm known to be nine years of age and at least five years of age respectively, (Bassett, *in. litt.* May 6, 1942) displayed fused nasal sutures and sagittal crests of over 40.0 mm. in length. The skulls of six marten from two litters known to be five and six weeks old (Pearson and Enders, 1944) showed faint temporal crests in parallel lines about one-third of the way up the parietals. The distance between the origin of the temporal muscles was much greater than for any of the wild caught animals, all of which were at least seven or eight months old.

Two direct comments on the development of the sagittal crests with age were found in the literature. Coues (1877:76) states: "The sagittal crest divaricates anteriorly to run out to each supraorbital process; in old animals it is a *high ridge*; in the young a *tablet* of greater or less width." While Grinnell et al (*op. cit.*) state of marten skulls (p. 187): "Changes that take place with advancing years, from youth to old age, include. . . . . development (in males) of well-marked sagittal . . . . . ridges."

From a consideration of the foregoing data it seems possible to distinguish between mature and immature pine marten of both sexes by use of the

presence or absence of the sagittal crest. Also male animals with a sagittal crest of less than 20.0 mm. are immature. Fortunately, this character can be readily examined in the skulls of skinned animals without further treatment, and trappers can save the skulls with a minimum of difficulty.

The author believes that, with more adequate series of bacula and ovaries with matching skulls as well as material of different known ages, it will be possible to determine the age of adult marten even more closely. On the basis of the materials presented here it seems possible to postulate that males with sagittal crests of more than 30.0 mm. and females with sagittal crests of more than 20.0 mm. may be termed "overmature" or "very old."

LITERATURE CITED

BRASSARD, J. A. and R. BERNARD. 1939. Observations on breeding and development of Marten, *Martes a. americana* (Kerr) Canadian Field. Nat., **53**(2): 15–21.

CAHALANE, V. 1932. Age variation in the teeth and skulls of the white-tailed deer. Cranbrook Inst. Sci., Sci. Publ. **2**: 1–14.

COUES, E. 1877. Fur bearing animals: A monograph of North American Mustelidae. U. S. Geol. Surv. of the Territories. Misc. Publ. **8**: 1–348.

DEANSELY, R. 1935. The reproductive processes of certain mammals: Part IX Growth and reproduction in the stoat (*Mustela erminea*) Philos. Trans. Royal Soc. London, Series B, 518, vol. **225**: 1–62.

ENDERS, R. K. 1939. Fur animal reproductive cycles and their relation to management. Trans. N. A. Wildl. Conf. **3**: 515–517.

GRINNELL, J. and J. S. DIXON and J. M. LINSDALE. 1937. Fur bearing mammals of California: Their natural history, systematic status, and relations to man. Univ. Calif. Press, Berkeley, Calif. 2 vols.

KOZANTSCHIKOV, I. 1930. Ein Beitrag zur Kenntnis der Oekologie, Biologie, und Geographie des Zobels, (*Martes zibellina* L.) veitschr. Wiss. Biol., Abt. A, Morph. u. Oekol. Tiere, **19**(2/3): 291–320.

MALLNER, F. 1931. Zur Biologie vom Zobel. Die Pelztierzucht u. Kleintierzucht, **7**(12): 219–223.

MARSHALL, W. H. and R. K. ENDERS. 1942. The blastocyst of the marten (*Martes*). Anat. Record **84**(9): 307–310.

PEARSON, O. P. and R. K. ENDERS. 1944. Duration of pregnancy in certain mustelids. J. Exp. Zool. **95**(1): 21–35.

PETERSON, C. G. J. 1895. Eine Methode zur Bestimmung des Alters und Wuchses der Fische. Mitt. Deutschen Seefischereivereins. **11**: 226–235.

POPOV, V. A. 1943. A new age index in *Mustelinae*. Comptes Rendus (Doklady) Acad. Sci. U.S.S.R., Moscow, **38**(8): 258–268.

SCHMIDT, F. 1934. Ueber die Fortpflangsungsbiologie von Siberischen Zobel (*Martes zibellina* L.) und Europaischen Baummarder (*Martes martes* L.) Zeitschr. für Saugetierk., **9**: 392–403.

VAN OOSTEN, J. 1929. Life history of the lake herring (*Leucichthys artedi* LeSueur) of Lake Huron as revealed by its scales, with a critique of the scale method. Bur. of Fisheries Doc. 1053: 265–428.

WRIGHT, P. L. 1937. The sexual cycle of the male long-tailed weasel (*Mustela frenata*). Jour. Mammal. **28**(4): 343–352.

Accepted for publication Dec. 5, 1950.

MISTAKEN DIAGNOSIS OF ACTINOMYCOSIS FOR OSTEOGENIC SARCOMA IN AN AMERICAN ELK (CERVUS CANADENSIS) [1]

Charles H. Drake

Dept. of Bacteriology and Public Health, Washington State College, Pullman, Washington

Actinomycosis, often called "lumpy jaw" was first described in cattle by Bollinger in 1877 and in the following year the disease was reported in man by Israel. Ponfick, in 1879, pointed out the identity of the two diseases. By the use of anaerobic techniques the causative organism, *Actinomyces bovis*, was isolated by Wolff and Israel in 1891. Unfortunately, in the same year the situation was confused by the report of Bostroem who made a cultural study of the disease and after many efforts isolated an aerobic form in a limited number of cases. In the light of present knowledge it is clear that Bostroems' organism was a contaminant from the soil and bears no relationship to the disease. However, this organism, improperly labeled *Actinomyces bovis*, has become widely distributed throughout culture museums all over the world. Microscopically this organism and the other soil species have certain similarities to the true anaerobic species causing the disease. Despite the work of Wright in 1905 who confirmed Wolff and Israel as to the anaerobic or microaerophilic nature of the causative organism, the situation remained badly confused as a result of Bostroems' work and the later observations that organisms similar to his could be readily isolated from the soil. An erroneous chain of evidence was built up by the correlation of several observations such as: the occurrence in cattle of the disease and the fact that the largest percentage of human cases were observed in farmers and agricultural workers, the observation that occasionally the initial lesions were found to contain a barley awn or a fragment of straw and were most common in the jaws of cattle and in the head and neck of human beings, the ready isolation of microscopically similar organisms from soil and vegetable materials. Although the organisms isolated from soil and from plants were not pathogenic, this was not looked upon as negative evidence since it is extremely difficult and often impossible to produce the disease by injecting material from true lesions. The theory was built up that actinomycosis was caused by the adaptation of soil organisms to growth in the tissues following their introduction on bits of straw or other vegetable material. The preponderance of agricultural workers as human patients was thought to substantiate this.

The above fallacious theory has been upset, however, by irrefutable evidence to the contrary and by other valid explanations of the supposed substantiating

[1] Publication no. 2 of the Cooperative Study of Diseases of Nature in Washington. This investigation was supported in part by funds provided for medical and biological research by the State of Washington Initiative Measure No. 171.

evidence. It is now apparent that the preponderance of agricultural workers as patients was due to the fact that the population group from which the figures were gathered was made up primarily of farmers and others in related occupations so that the patients simply showed a normal distribution with regard to occupation. Wolff and Israel and later Wright had shown that the organisms could be consistently and readily isolated in the absence of secondary contamination or infection by use of anaerobic techniques as compared to the rarity with which the aerobic strains, now known to be contaminants, were isolated. The presence of the anaerobic or microaerophilic, Wolff-Israel type, in the typical cases has been repeatedly shown by many recent workers. The correlation with bits of straw, barley awns or other vegetable matter in the lesions can be explained in another way together with the observation that in man the appearance of the lesions even more often follows dental surgery such as tooth extraction. This has been even further amplified by the repeated demonstration of the true *Actinomyces bovis* as a common parasite of the oral cavity having been found in the crypts of normal tonsils and in the film and deposits that naturally form on the teeth. Such reports have come from a number of workers such as Lord and Trevett, Emmons, Slack and Rosebury and the organism has been isolated from the normal mouth on two occasions by the author. It is now evident that the etiological agent is a common parasite of the normal mouth which, if it gains access to deeply injured tissue following tooth extraction or is carried in from the saliva on any object that penetrates into the tissues of the mouth, can set up infection rather rarely under exceptional circumstances.

The purpose of the above brief historical survey is to point out the problem which now arises. As long as soil was accepted as the origin of *Actinomyces bovis* the occurrence of the disease in a number of wild animals, such as mountain sheep and elk as has been reported, seemed to fit in well with the general idea of the disease. However, with the demonstration that the organism is a parasite, common in the human mouth, and has never been isolated except from the animal body the question immediately arises as to whether the organisms causing the reported actinomycosis in wild animals are the same as those infecting man and cattle or whether they represent new and different species. Indeed, the frequency with which the disease has been reported in mountain sheep (Green, 1949) seems to be unusually high as compared to the number of bovine and human cases, which suggests the possibility that a different species might be involved. We have been interested, and still are, in obtaining material for study on this particular point.

In February 1949 a representative of the Washington State Game Department, Mr. Gray, brought to the laboratory the head of an elk found dead in a winter feed lot. The right jaw was greatly enlarged with a projecting mass partially covered by skin but with the outermost portion raw and necrotic. A field diagnosis of actinomycosis had been made on the specimen and this was consistent with the gross appearance.

The mass was partially dissected in order to find the purulent necrotic centers characteristic of the infection but except for the raw surface with patches of necrosis and secondary infection no underlying pockets or sinus tracts were evident. Spicules and small masses of bone were encountered throughout the lesion interspersed with solid areas of fibrous tissue mixed with other areas of myxomatous tissue. Although the obvious new growth of bone encountered might suggest the spongy bone typically found in actinomycosis, "lumpy jaw", of cattle the mixture of bone with fibrous connective tissue in some areas and myxomatous tissue in others, in the absence of purulent areas and sinus tracts, much more strongly indicated an osteogenic sarcoma. This gross diagnosis was confirmed by removing a number of blocks of tissue from various portions of the mass so as to include the various tissue types found. The specimens were fixed in formalin and sections were prepared. Routine staining with hematoxylin and eosin was used. Microscopic study of the sections showed the lesion to be a typical osteogenic sarcoma of a rather low order of malignancy. The diagnosis by microscopic pathology was also confirmed by both Dr. D. R. Cordy, pathologist in the School of Veterinary Medicine, Washington State College and by Dr. R. Weiser of the University of Washington Medical School.

It is hardly possible to evaluate positively the relationship of this tumor to the death of the animal. Since the elk was found dead and several days elapsed between the time the head was removed and brought to this laboratory and information as to the nature of the lesion was available, a thorough autopsy in the field was not possible. Because of the conditions, a thorough examination was not possible but gross inspection of the lungs by Mr. Gray failed to show any metastases. This is not unusual in view of the grade of malignancy shown by sections of the primary lesion although in both human beings and lower animals sarcomas not removed surgically eventually metastasize with fatal results. In this case it is quite probable that the mere mechanical difficulties in procuring food plus the deleterious effects of secondary infection were sufficient to cause death of the animal before extensive metastases occurred.

The isolated demonstration of an osteogenic sarcoma in an elk is hardly a discovery of outstanding significance. Although we are not aware of any previous reported cases of such tumors in elk, they are known to occur in a variety of animals and in some species, strains are known in which such tumors are very frequent. Of greater importance, is the fact that this case illustrates the inherent dangers of field diagnoses when not confirmed by laboratory findings unless, of course, a field diagnosis is merely made on one of a series that has previously been examined and identified in the laboratory. Indeed, one might even raise the question as to how many previously reported cases of actinomycosis in wild animals actually represent the disease rather than some other condition, such as a tumor, which has given rise to a lesion with more or less superficial gross resemblance to "lumpy jaw". The question is even more to the point when one considers the number of diagnoses made on skeletal remains

found in the field. Either actinomycosis or osteogenic sarcoma can give rise to lesions with an irregular new growth of spongy bone and the true cause of such growths may be difficult or impossible to identify in the absence of the softer tissues or, if these are still present, if they are neither examined microscopically nor by cultural methods.

SUMMARY

1. A brief historical review of actinomycosis is given to show how the erroneous concept arose that this infection emanates from the soil, whereas it has now been shown that the organism is a frequent and usually harmless parasite of the oral cavity. This raises the question as to whether actinomycosis in wild animals, since it does not originate in the soil, is caused by the same or different species than that affecting man and cattle.
2. A lesion of the jaw bone of an elk, which had been diagnosed in the field as actinomycosis, was proven in the laboratory to be an osteogenic sarcoma.
3. Attention is called to the dangers of field diagnosis of diseases without laboratory confirmation in view of gross similarities of what may be very different disease entities of quite different implications.

LITERATURE CITED

BOSTROEM, E. 1891. Untersuchungen über die Aktinomykose des Menschen. Beit. path. Anat. **9**: 1–240.

BOLLINGER, O. 1877. Ueber eine neue Pilzkrankheit beim Rinde. Centralblatt fur d. med. Wissenschaft. **15**: 481.

EMMONS, C. W. 1936. Strains of Actinomyces isolated from tonsils. Puerto Rico J. Pub. Health and Trop. Med. **11**: 270.

GREEN, H. U. 1949. The Bighorn Sheep of Banff National Park. National Parks and Historic Sites Service, Development Services Branch: (Canada) Ottawa.

ISRAEL, J. 1878. Neue Beobachtungen auf dem Gebiete der Mykosen des Menschen. Archiv für path. Anat. und Psysiologie. **74**: 15.

LORD, F. T. and TREVETT, L. D. 1936. The pathogenesis of actinomycosis. Recovery of actinomyces-like organisms from the normal mouth. J. Inf. Dis. **58**: 89.

PONFICK, E. 1879. Über Actinomycose. Berl. klin. Wochenschr. 345.

ROSEBURY, T. 1944. The parasitic actinomycetes and other filamentous organisms of the mouth. Bact. Rev. **8**: 189.

SLACK, J. 1942. The source of infection in actinomycosis. J. Bact. **43**: 193.

WOLFF, M. and ISRAEL, J. 1891. Ueber Reincultur des Actinomycetes und seine Uebertragbarkeit auf Thiere. Arch. Path. **126**: 11.

WRIGHT, J. H. 1905. The biology of the microorganism of actinomycosis. J. Med. Res. **8**: 349.

Accepted for publication Nov. 30, 1950.

EXAMINATION OF A STATE FOX BOUNTY[1]

D. F. Switzenberg

Michigan Dept. of Conservation, Shingleton, Mich.

During recent years when the red fox (*Vulpes fulva*) attained its highest recorded population level, many states were hard pressed by sportsmen to do something drastic in the way of control. A bounty often has been the answer to such demands. Although the majority of state game departments have not favored bounties, mounting public pressure in times of predator population highs and coincidental declines in other game species often has forced their hand on the issue. Michigan's experience with bounties undoubtedly has been typical of many states.

Bounties usually have taken the form originally of county-financed systems in which the bountied animals have been presented to the county clerk or some other designated official for payment. After paying out of their own treasuries for some years, the tendency has been for county authorities to force the responsibility upon the state. Michigan was no exception in this respect. After the Department of Conservation resisted efforts to bring a bounty into force, the State Legislature in October, 1947 provided for a state-financed bounty on the red fox through earmarking of fifty cents from each small game license. With the inception of the new fox bounty, the state was now paying bounties on four species of predatory mammals—the timber wolf, coyote, bobcat and red fox.

[1]A contribution from Pittman-Robertson Research Project 39-R, Game Division, Michigan Department of Conservation.

The possible value of bounties as a tool in predator control has been investigated in many states. In Pennsylvania, Gerstell (1937) made a comprehensive study of that state's long-used system. Recently Pennsylvania has renewed its study of the bounty system. Jacobsen, (1945) in a California publication, surveyed bounty programs in operation in a number of states. Several other authors have written excellent reviews, both technical and popular, of the bounty system. The conclusions reached by all these authors were essentially the same, namely, that bounties are expensive, wasteful of public funds, conducive to fraud, and have not materially reduced numbers of the bountied species.

Although the above studies have provided considerable enlightenment on the workings of the bounty, few intensive studies have been made on the effect of any particular system on a specific predator. When Michigan's fox bounty was put into operation October 11, 1947, a timely opportunity was presented to make such a study. A Pittman-Robertson research project on the red fox was already in progress. Population trends and the effect of control methods on populations were being studied. The study thereafter was expanded to include particular emphasis on the workings of the new bounty.

Objectives of the Study. Specifically, we were interested in finding answers to the following questions: (1) Would the bounty materially reduce fox numbers?

(2) Would it produce results commensurate with its cost? (3) If it should aid in reducing fox populations, would the reduction result in an increased game supply?

To answer these questions it was first necessary to obtain certain basic data on the take of red foxes, particularly as to its geographical distribution, spread over the year, and methods by which foxes were taken. Provision was made at the outset to record this information on a form on which the trapper must certify, with notarized signature, that the hides presented for bounty have not been presented elsewhere.

As an aid in interpreting the pattern of bountying in Michigan, a very brief review of the political and economic structure of the state is given. Generally speaking, the state's industry, agriculture, and most of its human population are concentrated in the lower one-third of the state, and particularly in the eastern part of this third. North of this region the state is sparsely populated and much of it is in second-growth forest remaining from lumbering operations of many years ago. In the Upper Peninsula the tourist trade, lumbering, and mining form that area's chief resources. In the northern part of the Lower Peninsula, the tourist trade, fruit-growing, and various forms of agriculture are the chief activities.

For convenience in comparing the hunting and trapping kill, the state has been arbitrarily divided into three geographic zones. Zone 1 includes the Upper Peninsula, Zone 2, the northern portion of the Lower Peninsula north of "townline 16" (the north line of Township 16 North), and Zone 3, the counties south of this line. Although this is a division of convenience, each zone is characterized by a predominance of certain habitat, climatic, and geological features in addition to marked differences in economic makeup. All of these physical characteristics have a marked effect, not only in determining fox populations, but on the hunting, trapping and other "fox harvesting" habits of the state's inhabitants.

The following analysis is based on the two principal sources of information available during the period under consideration. These were (1) computed kill figures from hunters' and trappers' report cards, and (2) the first year's bounty figures. Michigan has employed a compulsory game kill card system since 1937 which has served as a reasonably accurate index to the annual take of game and fur-bearers. The bounty figures had the added advantage of being absolute rather than computed figures. They also made possible an area breakdown of the kill to the township level, as well as a chronological breakdown over narrow time intervals.

By correlating all the data from the above sources with previous knowledge as to the manner in which foxes customarily are taken in Michigan, it was possible to make a fairly accurate evaluation of any effect the bounty may have had on the kill. In considering the "fox-harvesting" pattern in Michigan it is necessary to make certain general assumptions based upon conclusions reached after considerable personal contact by the writer with trappers, hunters, farmers, sportsmen, and others who have occasion to handle foxes. These assumptions are employed throughout the discussion when applicable.

One such assumption involves the reasons usually given for taking foxes by the three leading methods discussed below. It has been shown that foxes taken by hunting, for instance, usually are killed for sport in connection with regular fox hunts, incidentally to other types of hunting, or because the foxes are molesting poultry or livestock. The profit motive is seldom a prime consideration in shooting foxes. Trapping, except where a nuisance animal is to be removed, is most often done for the fur involved, and usually with a financial gain in mind. The bounty merely replaces the value of the fur as a profit incentive when fur prices are so low as to discourage trapping. Removing the pups from dens is likely to be carried on for the bounty offered either by the county or the state.

DISTRIBUTION OF THE KILL

Examination of the first year's bounty payments from October 11, 1947 to October 10, 1948, showed a total of 29,484 foxes bountied throughout the state. Of this number, 5,276 were taken in Zone 1, 12,363 in Zone 2, and 11,845 in Zone 3. From the standpoint of area productivity, the average yield per section in each Zone was .31 of a fox in Zone 1, .85 in Zone 2, and .45 in Zone 3. The statewide average was .51, or in whole figures, about one fox per two sections.

On the basis of man-hours of effort expended, previous studies have shown that trapping is much more efficient as a control measure than is hunting. However, because of the constantly increasing interest in fox hunting for sport, and the fact that most small game and deer hunters will take a shot at a fox if the opportunity presents itself, many foxes are bagged in this way. The cumulative effect of all this shooting has been to raise the total kill by hunting over that by trapping. Only during the first year of the bounty, when trapping evidently was encouraged over previous years, did the computed number of foxes trapped exceed those bagged by hunters. First year bounty returns disclosed that 57.2 per cent of the foxes presented during that period were taken by trapping, 30.7 per cent by hunting, 10.6 per cent by den-digging, and 1.5 per cent by all other methods. (Figure 1).

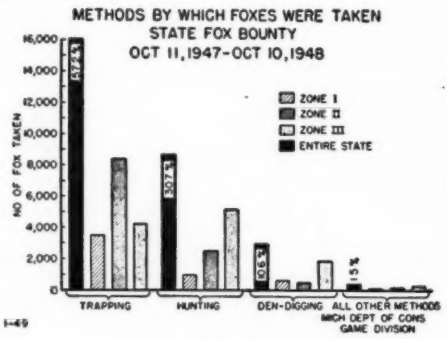

FIG. 1

A breakdown of the 1.5 per cent of foxes killed by other agents revealed that a sizeable number met death along highways. Bounty claimants reported a total of 322 foxes that were either struck by cars or picked up on the highway after being killed by other traffic. Mortalities from this source actually amounted to 1.1 per cent of all foxes bountied during the year. Such losses can be explained in part by the carrion-eating tendencies of foxes which prompt them to pick up road-killed animals for food. This habit and the normal amount of movement across roads evidently

constitute a mortality factor in years of peak abundance. Listed under the remaining 0.4 per cent of bountied foxes were animals taken by poisoning, dynamiting dens, shooting with bow and arrow, and by being caught in poultry enclosures, electric fences and mowing machines.

Examination of the kill in respect to its distribution over the year shows a marked fluctuation in numbers of foxes taken during different months (Figure 2).

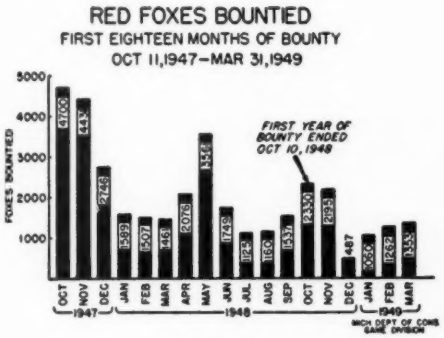

FIG. 2

In general, the heaviest take occurred during October and November when fur-trappers normally start operations and when many foxes are turned in by small-game hunters and deer hunters. The exceptionally high kill during these two months in 1947 may have been due partly to a backlog of summer hides left over by trappers and presented as soon as the bounty went into effect on October 11. This was implied by the fact that the total presented for bounty during October and November 1947, was more than double that for the same period in 1948 (Figure 3), after the bounty had been in force a full year.

Another period of high yield occurred during the spring months, particularly in May. Presumably this reflected foxes removed from the vicinity of dens by trapping, shooting, and den-digging. It was interesting to note that den-digging did not account for the majority of animals taken during this period as previously suspected. (See Figure 1). Recent evidence suggests that people have found fox pups can be removed from dens more easily and with better assurance of taking the last pup by trapping and shooting then by the laborious back-breaking application of pick and shovel.

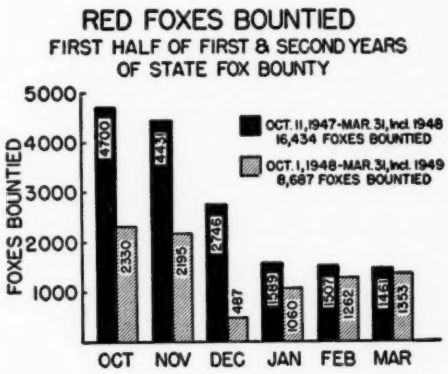

FIG. 3

The common practice of excavating the dens in spring should be condemned whole-heartedly by game managers. As an immediate and effective control measure, the tendency has been for state game agencies to condone it in years of peak fox abundance when they were under public pressure to employ drastic control measures. In the process of digging, however, many dens are destroyed which otherwise could be utilized by badgers, woodchucks, rabbits, and other game and fur-bearers. Excavation of ground dens, where pursued to excess, may actually reduce the

carrying capacity of the land for these species.

EFFECT OF THE BOUNTY ON THE KILL

The bounty became operative October 11, 1947, the date almost coinciding with the annual opening of the small game and trapping season in Michigan. Thus, by analyzing the computed kill figures from hunters' and trappers' report cards it was possible to make a comparison between the foxes killed in a year when a bounty was in operation (1947–48) and a year (1946–47) in which there was no bounty. Employment of these figures to ascertain any possible influence of the bounty presupposes, of course, that no other factors, man-made or natural, were of great significance at the time. In the analysis to follow, no such factors were detected which could have markedly affected the take.

Before examining kill returns from the past two years it should be pointed out that considerable evidence of a downward trend in populations was noted as early as the 1946–47 hunting and trapping season. The computed hunting and trapping kill (combined) dropped to a total of 39,652 in that season from an all-time high of 52,017 in 1945–46. Although the greatly reduced take in 1946–47 could have resulted partially from other causes, other data definitely support the evidence of reduced populations.

Analysis of computed kill figures disclosed that despite the bounty the combined take of foxes by hunting and trapping during the 1947–48 season showed a net drop of 2.4 per cent (See Figure 4).

The hunting yield declined at the rate of 19.4 per cent, which was almost identically the same rate of decrease that occurred between the seasons of 1945–46 (when a total of 52,017 foxes were bagged by hunting and trapping combined) and 1946–47. In marked contrast to hunting, the trapping take increased twenty-one per cent. If the same rate of decline in the trapping take (29 per cent) that occurred between the two above years had continued into 1947–48 approximately 4,855 fewer foxes would have been taken instead of the computed additional 3,508. Therefore, if we assume that without a bounty the drop in trapping would have been comparable to the drop in hunting, it becomes evident (1) that the bounty resulted in 8,363 (4,855 + 3,508) more foxes being trapped, and (2) that the bounty had no appreciable statewide effect on hunting, which experience has shown is usually little influenced by a bounty.

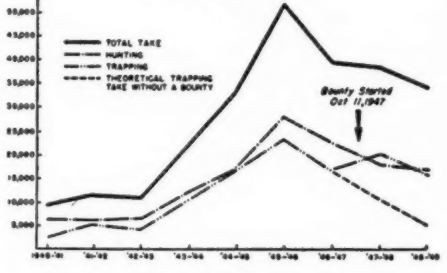

FIG. 4

Further analysis of both bounty and computed kill figures demonstrates certain zonal features of the bountying pattern. When the price of raw fox fur dropped to its lowest point in many years less inducement was offered to trapping for profit. To replace this loss

of revenue trappers generally added their voices to the demand for a statewide bounty which in effect would subsidize them at a time when fur trapping was not worthwhile. Thus, the state bounty became instrumental in reviving a waning occupation. Largely as a result of the bounty, the take of foxes increased in the northern part of the Lower Peninsula (Zone 2) where many trappers operate, but declined in the southern agricultural and industrial countries (Zone 3), where the financial motive was of less importance.

In Zone 2 both hunting and trapping were accelerated, but neither was materially influenced in Zone 3. The substantial increase in foxes taken in Zone 2 again bears out previous knowledge that this Zone comprises an area of high fox density and normally supports more fur "takers" than does either Zone 1 or 3 (Figure 5).

FIG. 5

In the Upper Peninsula (Zone 1) an entirely different situation prevailed. Here the fox kill has been shown by hunting and trapping kill figures to remain fairly constant from year to year, and to be less affected by economic conditions or the presence of a fox bounty than other parts of Michigan. In this part of the state coyotes are relatively abundant and bring a considerably higher financial return in the form of a $15 – $20 state bounty. Foxes always have been considered a nuisance by trappers because they frequently get into coyote sets, temporarily spoiling the sets for the larger carnivores. Thus the majority of foxes taken here are a by-product of coyote trapping, and would be trapped regardless of whether or not they were on the bounty list. To compensate them for the inconvenience and outright financial losses sustained, trappers in the Upper Peninsula also have been vociferous in demanding a fox bounty.

In 1947–48 the season's computed take from Zone 1 (Upper Peninsula) was only ten (10) more than in the previous year, or an increase of only 0.23 per cent. This rise was almost entirely by trapping since the hunting kill, which always has been insignificant in this part of the state, decreased slightly.

Computed kill figures in 1948–49, with the bounty still in operation, revealed a further decline in the statewide fox take. During this year the kill continued downward to 34,093, a net drop of 12 per cent from 1947–48. It also will be noted that after the novelty of the bounty had worn off the trapping kill reverted to its normal pattern by falling to a point slightly under the hunting kill. (Figure 4).

Compilation of records, just completed, for the second year of the state fox bounty (October 1, 1948 – September 30, 1949) introduces a possible discrepancy between bounty and computed kill figures for the similar period. These tend to contradict the overall conclusions as to the effect of the bounty drawn above.

Since computed kill figures for the same general period, based on hunters' and trappers' compulsory reports, quite clearly had indicated a reversion to the normal hunting-trapping ratio, it was expected that bounty figures for the same period, when compiled, would be in reasonably close agreement. Instead, they continued to show about the same proportion of animals taken by hunting and trapping as in the previous year, namely, a significantly greater number taken by trapping than by hunting.

This rather obvious discrepancy between figures produced by the two different sources of information might raise some question as to the validity of our computed kill figures. The chief unknown appears to be in what constitutes the exact period covered by any one year's reports. Whereas the period covered by the bounty records is definite and unquestioned, it has been impossible to gauge the exact limits of that covered by the other source. For example, it is believed that hunters who submit their report cards just before the February 15 deadline are inclined to report those foxes which were shot in the fall and winter season just past. On the other hand, the hunters who turn in their cards immediately after the upland bird season in the fall (and many do this) are more apt to include animals taken in the preceding season.

This time difference in reporting could account for considerable disparity between the computed kill and bounty figures for any one year. However, it would not affect a comparison of the computed kill between one year and another. The somewhat smaller hunting kill shown by the bounty figures also could result from the fact that many foxes killed by hunting (which is done largely for sport) are not bountied. Computed trapping figures are subject to even more variables in this respect since under the prevailing bounty and lack of interest in fur-trapping, foxes are no longer being taken only when their fur is prime, but are being trapped throughout most of the year.

These unknowns do not arise in game or fur-bearing species which may be taken legally only during a few weeks or months of the year. But they do occur in connection with such unprotected species as the red fox which may be trapped, shot, or dug out of dens at all seasons. Until such time as the period covered by each annual report can be more accurately defined, our computed kill figures will need to be interpreted with the utmost caution.

Although the most recent (1948–49) bounty records obviously render our previous conclusions as to the effect of the bounty somewhat less tenable, the fact that such a marked upswing in trapping success seemed to have occurred for a short period following the first year of the bounty still strongly suggests that the bounty served as a marked stimulus to trapping.

BOUNTY STATISTICS

A breakdown of the first year's bounty figures provided pertinent data

on the distribution of the kill in relation to the number of foxes presented by individual bounty claimants. Results of this breakdown are included below in the form of two frequency tables.

From the above it will be noted that the number of foxes bountied by any one person varied from 1 to 506. Table 2 (below) combines these individual categories into twelve major

TABLE 1.—FREQUENCY INDEX OF FOXES TAKEN PER BOUNTY CLAIMANT

Foxes Taken Per Person	Number of Persons	Total Take	Foxes Taken Per Person	Number of Persons	Total Take
1	5011	5011	48	2	96
2	990	1980	49	1	49
3	514	1542	50	1	50
4	379	1516	52	2	104
5	243	1215	54	3	162
6	190	1140	55	2	110
7	121	847	56	1	56
8	94	752	57	1	57
9	77	693	58	1	58
10	116	1160	59	1	59
11	50	550	60	3	180
12	43	516	61	3	183
13	22	286	65	1	65
14	34	476	66	1	66
15	29	435	67	2	134
16	25	400	68	3	204
17	24	408	69	1	69
18	14	252	70	1	70
19	9	171	71	1	71
20	12	240	72	1	72
21	10	210	78	2	156
22	11	242	79	2	158
23	7	161	81	2	162
24	8	192	82	1	82
25	5	125	84	1	84
26	7	182	87	1	87
27	7	189	96	1	96
28	2	56	100	1	100
29	8	232	101	1	101
30	6	180	102	1	102
31	7	217	103	1	103
32	4	128	105	1	105
33	7	231	109	1	109
34	3	102	110	1	110
35	4	140	111	1	111
36	3	108	128	1	128
37	7	259	149	1	149
38	5	190	154	1	154
39	6	224	155	1	155
40	5	200	158	1	158
42	7	294	176	1	176
43	1	43	317	1	317
44	2	88	379	1	379
45	2	90	426	1	426
47	3	141	506	1	506

Total Number of Bounty Claimants 8,194
Total Take of Foxes.....29,943*

* It will be noted that the figure given here is slightly higher than the figure previously quoted for the first year of the bounty. This resulted from the fact that the above was a later tabulation when all or nearly all bounty affidavits were complete.

TABLE 2.—RECAPITULATION OF THE FREQUENCY INDEX

Range (No. Foxes Taken)	Persons Taking Foxes in These Ranges		Foxes Taken in These Ranges	
	No. of Persons	% of Persons	No. Taken	% Taken
1–10	7,735	94.40	15,856	52.95
11–20	262	3.20	3,734	12.47
21–30	71	.87	1,769	5.91
31–40	51	.62	1,799	6.01
41–50	19	.23	851	2.84
51–60	14	.17	786	2.62
61–70	12	.15	791	2.64
71–80	6	.07	457	1.53
81–90	5	.06	415	1.39
91–100	2	.02	196	.65
101–150	9	.11	1,018	3.40
151 and over	8	.10	2,271	7.59

groups for ease of interpretation, and also includes percentages of foxes taken under each group.

In evaluating the data presented above it seems reasonable to assume that most of the persons bountying from one to ten foxes are those who take them by hunting or incidental trapping, animals that in nearly all cases would have been killed without a bounty. These animals constituted about 53 per cent of the total bountied. At the other extreme were persons bountying in excess of 100 foxes. There were 17 in this category of fox-trapping experts who made a professional effort to take foxes for the bounty offered. They accounted for 11 per cent of the total take. From this is left 36 per cent of the bountied foxes, some of which were certainly taken for other reasons than the bounty. Altogether, it seems reasonable to conclude that approximately 75 per cent of the foxes bountied were taken for sport, incidental to other trapping, or for reasons of predator control, rather than directly as a result of the bounty.

Based on the above information, the following conclusions can be drawn regarding the efficacy of the bounty in reducing fox populations:

1. Increased the statewide *trapping* take in 1947–48 despite the 2.4 net drop by hunting and trapping combined.
2. Increased the yield by trapping in the northern part of the lower peninsula where many trappers operate and where trapping for fur traditionally has been important.
3. Had very little influence on the take in the extreme northern and southern counties.
4. Had little statewide effect on hunting, which is done primarily for sport or predator control, except in the northern part of the lower peninsula where more people are "bounty-conscious."
5. Increased the combined statewide take 25 to 30 per cent over what it would have been if the previous downward trend had continued.

Other facts revealed in the operation of the bounty were:

1. About three-fourths of the foxes bountied were taken by persons who were not primarily interested in the bounty.

2. The great majority of persons (94.4 per cent) took from one to ten foxes during the year. Nevertheless this "amateur" group accounted for 52.95 per cent of the total take.
3. Although a number of highly skilled trappers (17) took more than one hundred foxes each, the total number of animals accounted for by these persons was still only 11 per cent of the total bounty take.

Frequency tables based on the second year of the bounty (October 1, 1948 – September 30, 1949) reveal a picture very similar to that produced by the first year's figures. Since percentile distribution of foxes taken in the various ranges shown in Table 2 was not significantly different, no further discussion will be included here regarding frequency of the kill during the second year.

THE BOUNTY AND GAME POPULATIONS

The justification for any bounty should be decided entirely upon the issue of whether or not it adequately controls the bountied species. Furthermore, if the bounty does result in such control, it should be clearly demonstrated that it is benefiting the prey species in question. Subsidizing of trappers to keep their "profession" alive when trapping for fur is unprofitable should not enter into a consideration of bounties—unless it has been proven beforehand that such trapping is both necessary and effective. A final consideration should be the cost of the bounty in relation to the possible benefits derived.

During the first year of the bounty the sum of $147,420.00 was expended on 29,484 foxes. If it is assumed that despite a 2.4 per cent drop in the combined kill of foxes by hunting and trapping in 1947–48, more foxes were taken by trapping with a bounty than without, the fact remains that nearly three-fourths would have been taken anyway. Following this line of reasoning further, it is evident that each fox is costing not the legally prescribed $5.00, but actually $20.00. Since it has not been demonstrated that high fox populations have been a major factor in the game shortage, the expenditure of nearly $148,000 in a year's time, at an actual rate of $20.00 per fox, hardly seems justified.

The question of whether or not a slight increase in the fox kill has aided game populations presents a difficult problem for analysis. If computed kill figures on several leading game (prey) species for the 1948–49 season (the first season immediately following the first year of the bounty) are cited, it will be found that the take of most of these species was increased. The pheasant, ruffed grouse, cottontail rabbit, snowshoe hare, and fox squirrel all showed definite increases. The same holds true for the muskrat, an important fur-bearer, which frequently is an item of some significance in the diet of foxes. On the basis of these figures alone, proponents of the bounty immediately would claim that the increased bags unquestionably resulted from the extra foxes taken because of the bounty.

It is not the purpose of this paper to become involved in a discussion concerning the causes of the highs and lows in game populations. Certainly those

biologists who have worked on the pheasant problem agree that a number of factors besides predation have caused the pheasant slump, and that the recent signs of recovery in Michigan and several other states could not have resulted primarily from any recent coincidental reduction in fox populations. Cyclic factors in the other species undoubtedly were of importance, and there is no evidence that changes in fox numbers could have had any great influence on their population trends.

The conclusions to be drawn from this discussion, while not entirely clear, are that the reduction in populations of foxes, and probably other predators also, has produced no observable effects in numbers of the above game species.

SUMMARY

1. A typical state bounty has been studied and its results in controlling the red fox have been analyzed. Similarly, its cost in relation to the extent to which it reduced the fox population and benefited game have been examined.

2. Computed kill figures from hunters' and trappers' report cards, and the first year's bounty totals have served as the principal sources of information on annual kill of foxes and game species.

3. During the first year of the bounty, 29,484 foxes were taken throughout the State. Distribution of the take was as follows: Zone 1 (Upper Peninsula)—5,276, or .31 fox per section, Zone 2 (north half of the Lower Peninsula)—12,363, or .85 fox per section, Zone 3 (south half of Lower Peninsula)—11,845 or .45 fox per section.

4. The bounty's influence on the various methods of take have been noted. It was found that trapping was increased on a statewide average, but that hunting was little affected except in the northern part of the lower peninsula.

5. The heaviest take of foxes occurred during October and November when fur trappers customarily start trapping, and when many foxes are submitted for bounty by small-game and deer hunters. A second period of high yield prevailed during the spring months when many young foxes were removed from dens.

6. Based upon the probable number of foxes which would have been taken if the previously established downward trend had continued, the bounty resulted in an approximately 26 per cent greater kill than would have occurred without a bounty. Although this conclusion was modified somewhat by an apparent discrepancy between computed kill and bounty figures during the second year of the bounty, it still appeared that the bounty noticeably stimulated trapping.

7. In the absence of any proof that the 26 per cent additional foxes taken because of the bounty have been a causative factor in the increased game supply, the expenditure of nearly $148,000 during the bounty's first year of operation did not seem justified.

8. Although the relationship is not entirely clear, the increase in the kill of pheasants, ruffed grouse, cottontail rabbits, snowshoe hares, fox squirrels, and muskrats during the 1948–49 season evidently did not occur largely as the result of a reduction in fox numbers. The increases noted probably were brought about by other factors, such as more favorable breeding seasons, cyclic trends in some species, and in-

creased hunting pressure, all of which have been evident to biologists for several years.

LITERATURE CITED

GERSTELL, RICHARD. 1937. The Pennsylvania Bounty System. Research Bulletin No. 1. Pennsylvania Game Commission. Harrisburg.

JACOBSEN, W. C. 1945. The Bounty System and Predator Control. Reprint from California Fish and Game, April 1945, Vol. 31, No. 2. San Francisco.

Accepted for publication October 7, 1950.

A TWO YEAR STUDY OF PHEASANT STOCKING[1] IN THE GALLATIN VALLEY, MONTANA

Edwin F. Roby

Box 474, Bozeman, Montana

INTRODUCTION

The annual stocking of game farm pheasants has long been practiced in Montana. Findings in other states (Gerstell 1936, Buss 1946, Ginn 1947, Pushee 1948, McNamara 1949) cast considerable doubt on the advisability of this practice when the results are analyzed on the basis of "returns" in the hunter's bag. A detailed small scale study was conducted in Gallatin County to secure survival data on game farm pheasants released in that area. These data are evaluated on the basis of methods of release, habitat where released, and the relative importance of "returns" in the hunter's bag when compared with birds produced naturally in the field.

It is impractical to list all who aided in the study but the writer wishes to extend his appreciation to the following: Don C. Quimby for advice and guidance during the field work and preparation of the manuscript; Clifford V. Davis for suggesting the problem and aid on many occasions; the Montana State Fish and Game Department for financial assistance, materials, and the advice of their technical staff, especially W. K. Thompson and W. H. Bergeson; my father, George W. Roby, for helping with the construction of release pens; W. D. Brandt, H. S. Hecox, Arthur Buckley, and H. B. Manley for permitting me to conduct investigations on their property.

[1]A joint contribution from Montana State College, Agricultural Experiment Station Project No. 844–42, Paper No. 236, Journal Series, and Wildlife Restoration Division, Montana Fish and Game Department.

THE STUDY AREA

The area selected for the study is an agricultural area northwest of Bozeman in the Gallatin valley. General land use practices and the natural topography make the area well suited for pheasants. Much of the land is used for the production of cereal grains providing a food supply (Hiatt 1947). Excellent cover, well interspersed with grain fields, is found along the banks of the many streams, roadsides and numerous small swampy areas. Willow (*Salix sp.*), chokecherry (*Prunus melanocarpa*), rose (*Rosa fendleri*), cat-tail (*Typha latifolia*), and hawthorn (*Crataegus sp.*) are found along roadsides and stream banks.

The many fence rows contain Canadian thistle (*Cirsium arvense*), gooseberry (*Ribes sp.*), rose (*Rosa fendleri*), and tall grasses. In some of the more open areas much sweet clover and alfalfa are found.

Four release sites were selected as follows: release site 1 is in an abandoned farm yard and pasture, 200 by 800 feet, located in the northeast corner of the study area about 5.75 miles northwest of site 2, 3 miles northeast of site 3 and 3 miles east of site 4. The vegetation includes quack grass (*Agropyron repens*), bluegrass (*Poa pratensis*), needle and thread (*Stipa comata*), western wheatgrass (*Agropyron smithii*), wavy leaved thistle (*Cirsium undulatum*), yarrow (*Achillea millefolium*), and fanweed (*Thlaspi arvense*). In the farm yard itself and around the buildings there are cottonwood (*Populus angustifolia*), apple (*Malus sp.*), rose (*Rosa fendleri*), loco (*Oxytropis lambertii*), and wild licorice (*Glycyrrhiza lepidota*).

Release site 2 is in a pasture 5.75 miles southeast of site 1, 3 miles east of site 3 and 7.5 miles southeast of site 4. The vegetation includes willow (*Salix sp.*), Canadian thistle (*Cirsium arvense*), rose (*Rosa fendleri*), yarrow (*Achillea millefolium*), aspen (*Populus tremuloides*), chokecherry (*Prunus melanocarpa*), snowberry (*Symphoricarpos occidentalis*), downy chess (*Bromus tectorum*), mountain brome (*Bromus magrinatus*), timothy (*Phleum pratense*), western wheatgrass (*Agropyron smithii*) and *Poa* sp.

Release site 3 is also in a pasture situated 3 miles south of site 1, 3 miles west of site 2 and 4.2 miles southeast of site 4. The area contains the following plants: Canadian thistle (*Cirsium arvense*), yarrow (*Achillea millefolium*), sweet clover (*Melilotus alba*), iris (*Iris missouriensis*), bluegrass (*Poa pratensis*), western wheatgrass (*Agropyron smithii*), downy chess (*Bromus tectorum*), snowberry (*Symphoricarpos occidentalis*), willow (*Salix sp.*), cottonwood (*Populus angustifolia*), rose (*Rosa fendleri*) and poison hemlock (*Conium maculatum*).

Release site 4 is 3 miles west of site 1, 7.5 miles southwest of site 2 and 4.2 miles north of site 3. The vegetation in the area includes cat-tail (*Typha latifolia*), sedge (*Carex sp.*), poison hemlock (*Conium maculatum*), rose (*Rosa fendleri*), thistles (*Cirsium arvense*) and (*Cirsium undulatum*), willow (*Salix sp.*), cottonwood (*Populus angustifolia*), snowberry (*Symphoricarpos occidentalis*), gooseberry (*Ribes setosum*), orchard grass (*Dactylis glomerata*), timothy (*Phleum pratense*) and foxtail (*Hordeum jubatum*).

A release pen 50 by 25 by 4 feet similar to those described by Kozlik (1948) was constructed at each release site. The sides consisted of 2 inch mesh wire; the top of camouflage netting which provided shade. For protection against predators, an additional strip of 1 inch mesh wire was placed around the bottom to a height of about 18 inches. A single electrically charged wire was also placed at this height on two of the pens. A shelter was constructed in each to provide protection from inclement weather.

METHODS

Five hundred and fourteen 10 week old birds were used in the experiment. On August 16, 1948, 96 birds were released at release site 1; 45 ♂♂ by violent release method (no pre-release

treatment) and 41♂♂ and 10♀♀ by the gentle release method (Kozlik 1948). In this method the pheasants were confined in release pens for two weeks where they were fed and watered regularly. Then the sides of the pen were elevated to allow the pheasants to escape gradually into the wild. In 1949, 418 birds were used in the study but only 375 (289♂♂) were actually released into the wild. Forty-three died during the 2 week confinement in the release pens. This heavy loss was thought to be due to the poor condition of the birds when received rather than to any condition associated with confinement. No losses occurred in the release pen in 1948 when the birds received were in much better condition.

The birds were released August 29 as follows:

Release Site	Violent Release		Gentle Release	
	♂♂	♀♀	♂♂	♀♀
1	55	5	32	14
2	55	5	28	16
3	39	8	17	18
4	46	4	17	16

The figures for violent release include 20 birds that escaped prematurely from the release pens.

Four methods were used to determine survival and movement; observations, hunter checking stations, trapping, and evaluation of band reports voluntarily returned by hunters.

All birds were marked and banded previous to release. In 1948 koroseal tags 2 by 3 inches were attached to the back of the birds' necks by means of a silver plated safety pin inserted through about ¼ inch of skin (Taber 1949). Red tags were used on those released by the gentle method, yellow on the others. In 1949 the birds were marked by painting the back of the head and neck with enamel. Eight colors, red, yellow, citron green, orange, orchid, dark blue, light blue, and dark green were used to identify the birds according to method and site of release. A month experiment with 27 captive birds indicated the superiority of these enamels for permanency and visibility as compared to Rhodamine B extra, white side-wall paint, metallic aluminum paint, and yellow sheep marking paint. Daily observations in the release areas were made for 31 days in 1949 and 14 days in 1948 following releases. The cooperation of land owners in the immediate vicinity was solicited and prepared forms were supplied for recording observations.

Five hunter checking stations were set up at various strategic points in the valley and operated on week ends for 5 days during the hunting season (Oct. 30 to Nov. 13, 61 days after release) in 1949. The stations were located so as to intercept the majority of hunters from the area. The majority of hunters using the area come from the south and east. The area to the north is mountainous, and there are few through roads. The kill of all hunters checking through the stations was examined. No checking stations were operated during the 1948 hunting season (Oct. 31 to Nov. 11, 75 days after release).

During the winter of 1950, 341 trap days (one trap one day) were completed between January 14 and March 16 The traps used were of the type described by Kutz (1945). They were placed in areas of pheasant concentrations and frequently moved. Wheat and mixed grains were used for bait.

RESULTS

Dispersal From Release Sites

Several birds were seen in the vicinity of the release pen in 1948 following release but too few data were recorded for quantitative analysis. In 1949 observations were intensified to determine the rate of dispersal of the pheasants released by the two methods. Eight hundred and thirty observations of marked birds seen in the vicinity of the release sites were recorded as sight records. Representatives of each of the 8 groups of marked birds were easily distinguished through the 21st day. From the 22nd to the 32nd day some difficulty was encountered in distinguishing the birds marked with the red and yellow. After 31 days, all markings were of limited value. The average number of birds from each release seen daily for each of the five weeks (only the first 3 days of the 5th week) following release are expressed as the percent of the total number released by each method. The first week after release there was an average of 13.6 per cent of the violent and 20.3 per cent of the gently released birds observed in the areas daily. For the remaining weeks the percentages were as follows: second, 5.7 per cent violent, 7.7 per cent gentle; third, 6.4 per cent violent, 5.2 per cent gentle; fourth, 3 per cent violent, .9 per cent gentle; fifth, 2 per cent violent and .6 per cent gentle.

For the gentle release birds, the data definitely indicate a gradual dispersal from the release site. The same trend is suggested for the violent release birds. The data are not readily interpreted so far as comparing the dispersal rates of the two groups are concerned. During the first two weeks when about 73 per cent of the sight records were recorded, gentle release birds were seen more often in the vicinity of the release sites. During the last 3 weeks, when only 27 per cent of sight records were recorded, the opposite was true. The fact that most of the sight records were recorded during the first two weeks would seemingly make that period most important for interpreting dispersal rates for the two groups. Furthermore, it would seem that some of the birds from each group would settle in the release area. If so, they could be represented in the sight records. Although the data available are not too conclusive, they suggest a slower dispersal rate for the gentle release birds. A later section on movements supports this conclusion.

Band Returns

Return of bands from birds taken during the 1948 hunting season was on a voluntary basis but local publicity was given the project. Only cocks were killed. There were 11 bands returned or 12.8 per cent of the total cock pheasant release. This total return consisted of 6 (13.3 per cent of violent release cocks) and 5 (12.2 per cent of gentle release cocks). One female was found dead on a highway. One cock (1.2 per cent of the total cock release) was returned during the 1949 hunting season.

In 1949 when checking stations were used on week ends and trapping was conducted during the winter, bands from 42 cocks (14.5 per cent of total cock release) were returned. Thirty-one were violent and 11 were gentle release birds. These violents represented 15.9 per cent of the violent cock release and the gentles were 11.7 per cent of the

total gentle release. By disregarding the trapping data a comparison with the 1948 figures can be made. Thirty-six cocks (12.5 per cent of total cock release) were taken by hunters. Twenty-seven were violent and 9 were gentle release birds representing 13.8 per cent and 9.6 per cent of the total releases respectively. These figures do not differ greatly from the 1948 figures suggesting that survival was about the same. The data further suggest that the voluntary return of bands in 1948 was about as effective as the use of checking stations in 1949.

Of the 87 cocks released at site 1 in 1949, 12 (13.8 per cent) were returned by hunters. Eleven (13.2 per cent) of the original 83 from site 2; 10 (17.9 per cent) of 56 from site 3 and 9 (14.3 per cent) of the 63 released from site 4 were returned by hunters or trapped during the winter.

Twelve, 1 hen and 11 cocks (3.8 per cent of the total cock release), were found dead from the time of release to the end of the hunting season.

For the two years the per cent band return (hunters and trapping) from released cocks was 14.4 per cent. A comparison of these results with some others obtained in other states reveals that the low rate of return for the present study is not inconsistent with results obtained elsewhere (see Gerstell, 1938, Buss, 1946, Ginn, 1947, Pushee, 1948, McNamara, 1949). All available information indicates a low return from artificially stocked pheasants. This fact need not be discussed here as it has been adequately covered by other authors.

The data indicate no marked differences in the survival rates of pheasants released by the two methods. For both years a higher percentage of violent release birds were recovered than gentle release birds (see above) but no particular significance can be attached to the small differences because of the small number of birds involved. Seemingly the survival rate for the two groups was somewhat the same. These data do not conform with the figures and statements of Kozlik (1948). From his work in Wisconsin he secured returns from gentle release birds as follows: 22 and 24 per cent from August releases of 6-week old birds and 51 per cent from full grown cocks released in October just a few days before the hunting season. Although he does not give figures in support, he concludes, "All evidence gathered in Wisconsin indicates that pheasants stocked from the gentle release pen have an increased survival." Figures published by Kozlik (1949) at a later date do not substantiate this earlier statement. His figures indicate a return of 180 and 179 respectively of crate and pen released birds when 307 ten-week old birds were released by each method in August. Further data by Kozlik (personal communication) likewise show no marked difference in survival rate of gentle and violent release birds. In August 1949, 306 juvenile cocks were stocked by each of the methods. The per cent return was 49 and 56 per cent respectively for the violent and gentle methods. These later findings of Kozlik corroborate the findings of the present study so far as comparative survival rates of pheasants released by the two methods is concerned.

Because of the small number of pheasants and the small area involved in the study it is difficult to correlate survival with the place of release. Al-

though all the release areas are somewhat different all are included in the same general type. The data show a slight predominance in the hunter take from birds released at site 3 (17.9 per cent of cock release). The returns from the other areas were site 4 (14.3 per cent), site 1 (13.8 per cent), and site 2 (13.2 per cent). In the absence of quantitative measurements of good pheasant cover it is difficult to state which of the release areas possess the best cover. Release site 1 has the least ground cover in the way of brush and dense herbaceous vegetation. Each of the three other areas have much brush and dense herbaceous vegetation in the immediate vicinity. Inasmuch as only one of these, site 3, apparently had a greater survival than site 1, the data do not reveal sufficient information for conclusions regarding survival at the four release sites.

A possible cause for the slight predominance of returns from site 3, could be due to the fact that this area is extremely well liked by hunters and is easily accessible from nearby towns. The relationship of habitat to survival has been discussed at some length by Gerstell (1938) and McNamara (1949). They concluded that the highest returns came from areas possessing better pheasant range in Pennsylvania and New Jersey.

Artificial Stocking vs. Natural Reproduction

The checking station data for 1949 supplies a reasonably large sample of the source of pheasants in the hunters' bag. During the five days that the checking stations were in operation 493 pheasants killed by hunters were checked. Twenty-one (4.3 per cent) were banded in August 1949. These data indicate that game farm pheasants supplied only a small part of the hunter take. This conclusion is justified because all pheasants released in the area in 1949 were banded. In light of the work of Buss (1946) the possibility of a game farm pheasant losing a leg band seems remote. He states, "To prove or disprove the allegation that pheasants lose aluminum butt-end bands, 1,000 mature birds were banded at the Wisconsin State Experimental Game and Fur Farm, brailed, and released during the fall of 1940 in 40-acre fields planted to corn and grown to weeds and scrub oak. When checked in the spring of 1941 these pheasants showed no band losses." The possibility of any of these birds being of game farm origin from releases earlier than 1948 is very small. McNamara (1949) from his own work and the work of others, states, ". . . the survival of pen-reared pheasants from the first to the second hunting season based on bands returned from 27,592 birds is less than one per cent."

An analysis of the age classes of the birds checked at the station supplies additional information on the importance of natural reproduction. Wildlife students from Montana State College were employed. They were instructed in methods of determining age classes. Three methods were used: examination for presence of bursa (Gower 1939, Linduska 1943, Kirkpatrick 1944, others), in which its presence indicates a juvenile; the use of the age gauge, (Kimball 1944) in which the age is determined by the length of the spur. Birds with spurs less than 3/4 inch long are considered juveniles; and the mandible test of Linduska (1945) which in-

volves lifting the bird by the mandible to determine whether it will support the weight of the bird. This method determines the amount of ossification of the skull. A soft mandible which breaks indicates a juvenile bird. The checking station operators were instructed to be extremely careful in all observations and to record all questionable data as such. They were further instructed to make all three tests on birds only when it was possible to do so without delaying hunters during rush hours. For this reason data are incomplete for some. In some instances the only information secured was whether the bird was banded. All birds were checked for bands. Even in cases where data are complete, the three methods did not always give uniform results. In questionable cases, the presence or absence of the bursa was considered sufficient for determining the proper age class. Kirkpatrick (1944) states that the presence of the bursa is a positive indicator of juvenile birds. Where the bursa character was not known a combination of the spur and the mandible tests were used.

Of the 493 birds checked, 331 (67.1 per cent) were wild juveniles, 21 (4.3 per cent) were game farm juveniles, 119 (24.1 per cent) were wild adults, and 22 (4.5 per cent) were wild birds of unknown age classes. These data clearly indicate that the majority of birds taken by the hunter were juveniles and only a minor proportion of these were game farm birds. The ratio of juvenile to adult was 2.9 to 1. The ratio of wild juveniles to wild adults was 2.7 to 1. Allen (1947) published data indicating birds of the year make up the important part of the hunter take. The ratio of adult to juveniles ranged from 1:2.6 to 1:16.3 for six hunting seasons at the Rose Lake Experiment Station in Michigan.

Movements From Release Sites

Band returns on 48 pheasants released in 1948 and 1949 provided information on the movement of artificially stocked birds. The information was obtained from hunters, by trapping, and from reports accompanying band returns. The exact location where banded birds were taken was not always definitely established. For this reason, the data are grouped into convenient categories.

Of the 48 birds, 20 (41.7 per cent) moved less than one mile, 24 (50 per cent) moved from 1 to 3 miles and 1 (2.1 per cent) moved from 4 to 5 miles. The remaining 3 (6.2 per cent) moved from 9 to 14 miles. Most of these movements were accomplished between the time of release and the end of the hunting season (62 to 76 days). Two, killed by cars, had moved 1.5 miles in 18 days. The 6 trapped were caught about .5 miles from the release site 192 to 199 days after release. The maximum movement, 14 miles, was accomplished in 62 days. The data are few but indicate that more gentle release birds settled within a mile of the release sites than violent release birds (56.2 and 34.4 per cent respectively). More violents (56.2 per cent) moved 1 to 3 miles than gentles (37.5 per cent). For movements of 4 to 14 miles the violents were also in the majority. Observations, discussed under dispersal, verify these findings.

These data compare favorable with those of McNamara (1949). He studied the drift of 315 male pheasants. About 50 per cent were shot within one mile

and 98 per cent within ten miles of the release sites. The maximum drift was 23 miles. Wandell (1945) on returns of 881 male pheasants, found that 65.9 per cent moved less than one mile and 96.4 per cent drifted less than 10 miles from the point of release.

SUMMARY

1. A detailed small scale study was conducted in Gallatin County, Montana 1948–50 to secure survival data on game farm pheasants released in that area.
2. The data are evaluated on the basis of dispersal from release sites, band returns, band returns as influenced by method and place of release, relative importance of banded pheasants in the hunters' bag, and movements of game farm pheasants.
3. Four - hundred and seventy - one game farm pheasants were released at four release sites by the gentle and violent release methods.
4. The data suggest a slower dispersal rate for the gentle release birds.
5. Band returns secured by the use of checking stations, traps, and hunter reports indicated a 14.4 per cent return of released cocks.
6. No marked difference in the survival rate of gentle release pheasants and violent release pheasants was noted.
7. Only 21 (4.3 per cent) of 493 pleasants checked at hunter checking stations in 1949 were game farm birds. Of the remainder 331 (67.1 per cent) were wild juveniles, 119 (24.1 per cent) were wild adults, and 22 (4.5 per cent) were wild birds of unknown age classes.
8. Movements from release site to point of kill for 48 birds was determined. Twenty (41.7 per cent) moved less than 1 mile, 24 (50 per cent) moved from 1 to 3 miles, 1 (2.1 per cent) moved from 4 to 5 miles, and 3 (6.2 per cent) moved 9 to 14 miles. A greater percentage of gentle than violent release birds settled in the immediate vicinity of the release sites.

LITERATURE CITED

ALLEN, D. L. 1947. Hunting as a limitation to Michigan pheasants. Jour. Wildl. Mgt., **11**(3): 232–243.

BUSS, IRVEN O. 1946. Wisconsin pheasant populations. Wisc. Cons. Dept. Publ. No. 326.: 1–184.

GERSTELL, RICHARD. 1938. An analysis of the reported returns obtained from the release of 30,000 artificially propagated ring necked pheasants and bob white quail. Trans. No. Am. Wildl. Conf. **3**: 724–729.

GINN, WM. E. 1947. Band returns from Indiana club reared pheasants. Jour. Wildl. Mgt., **11**(3): 226–231.

GOWER, W. CARL. 1939. The use of the bursa of Fabricius as an indicator of age in game birds. Trans. No. Am. Wildl. Conf. **4**: 426-430.

HIATT, R. W. 1947. The relation of the pheasant to agriculture in Yellowstone and Big Horn river valleys of Montana. Mont. State Fish and Game Comm.: 1–72.

KIMBALL, JAMES W. 1944. Age gauge for pheasants. Jour. Wildl. Mgt., **8**(3): 263–264.

KIRKPATRICK, C. M. 1944. The bursa of Fabricius in ring necked pheasants. Jour. Wildl. Mgt., **8**(2): 118–129.

KOZLIK, FRANK M. 1948. Gentle release method of stocking pheasants. Wisc. Cons. Bull., **13**(5): 1–3.

1949. The 1948 hunters check on Potter's Marsh. Wisc. Cons. Bull., **14**(3): 1–3.

KUTZ, H. L. 1945. An improved game bird trap. Jour. Wildl. Mgt., **9**(1): 35–38.

LINDUSKA, J. P. 1943. A gross study of the bursa of Fabricius and cock spurs as age indicators in the ring necked pheasant. Auk., 60(3): 426–437.

1945. Age determination in the ring necked pheasant. Jour. Wildl. Mgt., 9(2): 152–154.

MCNAMARA, L. G. and E. L. KOZICKY. 1949. Band returns from male ring necked pheasants in New Jersey. Jour. Wildl. Mgt., 13(3): 286–294.

PUSHEE, GEORGE F. JR. 1948. Summary report pheasant banding studies 1943 to 1948. Research Bull. No. 9, Mass. Dept. of Cons.: 1–22.

TABER, Richard D. 1949. A new marker for game birds. Jour. Wildl. Mgt., 13(2): 228–231.

WANDELL, W. N. 1945. Results of the 1943 and 1944 pheasant banding studies. Research Bull. No. 4, Mass. Dept. of Cons.: 1–22.

Accepted for publication October 6, 1950.

THE WILDLIFE SOCIETY

REPORT OF SECRETARY FOR YEAR 1950

The Society showed a steady growth during 1950 with the addition of 274 new members of which 229 were associates and 45 were actives. This does not include old associates who acquired active status during the past year. The total membership, as of December 31, 1950, was 1,743, of which 670 were actives and 1,058 associates. Fifty-eight new subscribers were placed on the mailing list for the Journal of Wildlife Management, making the total number of subscribers for the year 272. Subscriptions are now going to every State, Alaska, Hawaii, Canada, Hong Kong, Brazil, Australia, France, England, Denmark, Belgium, British West Indies, Russia, Philippines, South Africa, New Zealand, Sweden, and Indonesia.

There were still 139 individuals who were members in 1949 that have permitted themselves to be dropped from the Society membership as of January 1, 1951. Past experience has shown that most delinquents are due to negligence rather than intent. It is therefore, hoped that most of these individuals will eventually come back into the Society.

Resignations were received from 12 members at the close of the calendar year. Some of these were due to the new classification of members, but most of them offered other reasons.

It is with regret that it is necessary to report the deaths of three members during the year. These were: Dr. Rudolf Bennitt, of Columbia, Missouri, who died February 2, 1950; Donald G. Lauridson, of Meeker, Colorado, who died March 17, 1950; and Dr. Louis B. Bishop of Pasadena, California, who passed away April 3, 1950.

During the past year, the Secretary has mailed out approximately 4,000 pieces of correspondence which includes letters, post cards, invoices, ballots, and Journals. Thanks are hereby extended to Mrs. Geraldine Ewing, my secretary, for the splendid assistance she has contributed in keeping the Secretary's job in operation. She has been particularly appreciated during my absences while on field work and has made it possible for the members to receive much more prompt attention than would otherwise have been the case.

The Ballot Committee for 1950, consisting of Logan J. Bennett, Chairman, Cecil S. Williams, and Carl R. Eklund,

audited the constitutional revision ballots as well as nomination and election ballots. The results in the succeeding three paragraphs were extracted from the Committee's signed report in the Secretary's files.

Ballots were mailed out to the active members for their reaction to proposed constitutional amendments during the year. In brief, these were (1) that the newly elected officers and regional representatives take office at the end of the annual meeting each year instead of May 1, as was previously provided in the Constitution. (2) That the outgoing President be automatically elected to the Council for the year following his term as President. (3) That the associate member classification be eliminated in favor of a single type membership all of whom will be active and eligible to vote and hold office. The annual dues to be $5 per year for all members. All three amendments passed by majority vote as required by the Constitution and were incorporated into the Constitution which was reprinted in the October 1950 issue of the Journal of Wildlife Management.

The election ballot this year placed Warren W. Chase and Harlow B. Mills for President; Daniel Leedy and George Petrides for Vice-President; Irven O. Buss and W. C. Glazener for Secretary; Durward Allen and Allan Studholme for Representative in Region I; Leonard E. Foote and Clarence Watson for representative in Region II; Thomas Evans and William Barnes for representative in Region III; Lee Yeager and John Gottschalk for Representative for Region IV; Lee Arnold and W. B. Davis for Region V; Ben Glading and Starker Leopold for Region VI; and Charles D. Fowle and Ward E. Stevens as Representatives for Region VII.

The winners in this election were: Warren Chase, President; Daniel Leedy, Vice-President; Irven O. Buss, Secretary; Durward L. Allen, Representative for Region I; Leonard E. Foote, Region II; Thomas R. Evans, Region III; Lee Yeager, Region IV; Lee Arnold, Region V; Ben Glading, Region VI; and Charles D. Fowle, for Region VII.

Irven O. Buss notified the Secretary that he would be unable to accept his election as Secretary as he was being recalled to active duty with the Navy at the end of January. Through Council action, W. C. Glazener was appointed to this post and, therefore, will be the Secretary during the next two years.

The 10-year index was off the press during the year and 618 copies have been ordered by members and subscribers.

The October issue of the Journal of Wildlife Management was the last to be published by the George Banta Publishing Company of Menasha, Wisconsin. We were able to get the Cayuga Press of Ithaca, New York, to take over this important task. Their bid was the lowest received and we have the advantage of the Editor's presence in the same town to get them operating smoothly. Shipping all addressograph plates, back issues, and files to the new publisher plus their newness to the requirements of our Society have necessarily delayed the appearance of the January issue. It is the plea of the officers of the Society that the membership bear with us until the new publisher gets familiar with our Journal and the details we require.

Respectfully submitted,

March 5, 1951

SHALER E. ALDOUS, *Secretary*

REPORT OF TREASURER FOR YEAR 1950

A statement of cash receipts and disbursements for the year ending December 31, 1950, is as follows:

Balance, January 1, 1950		$10,568.91
Receipts—		
Members Dues	$7,818.82	
Subscriptions	1,766.00	
Back Issues	1,259.00	
Dividends & Interest on Investments	180.70	
Index sales	485.00	11,439.52
		$22,008.43
Disbursements—		
Printing of Journals	$6,377.50	
Printing of 10-year Index	1,199.60	
Printing, (other) Newsletter	536.74	
Reprints	41.78	
Stationery and printing	331.66	
Postage	132.58	
Express	22.23	
Office Equipment	13.25	
Clerical assistance	69.30	
Editor's expense	8.72	
Audit, Treasurer's Bond	72.50	
Refunds, Bank Charges, Bad Checks	41.64	
Miscellaneous	5.38	
Contributions (Natural Resources Council)	50.00	8,902.88
Balance, December 31, 1950		$13,105.55

Accountable as follows—

Cash balance in checking account
The Wildlife Society
Fulton National Bank
Atlanta, Georgia

Disbursements were reconciled to the checks issued by the Treasurer and as of December 31, 1950, the following checks were outstanding.

Date	Check No.	Payee	Amount
December 19, 1950	40	Geo. Banta Pub. Co.	$1,749.71
December 28, 1950	41	Alice Coker	15.00
			$1,764.71

The following represents the bonds, common stock, fidelity bond, and insurance policies.

United States Savings Bonds, Series "G", maturing in 12 years from date of issue, bearing interest at the rate of 2½ percent per annum:

Bond Number	Date Issued	Cost	Redemption Value 12/31/50
D2576621G	April 1947	$500.00	$474.00
D2576622G	April 1947	500.00	474.00
D2943626G	September 1948	500.00	481.00
D2943627G	September 1948	500.00	481.00
		$2,000.00	$1,910.00

Common Stocks—

	Date Issued	Certificate Number	Number of Shares	Cost
Loomis-Sayles Mutual Fund, no-assess & without par	8/25/48	2950	30	$992.70
State Street Investment Corp. no-assess & without par	1/8/48	C95210	22	979.00

	Present Value
Loomis-Sayles Mutual Fund	$1,107.30
State Street Investment Corp.	1,375.00

Fidelity bonds on the Treasurer of the Society carried with the General Casualty and Surety Company—Bond Number 425, in the amount of $10,000.00. Attention is called to the increase in bond from the amount of $2,500.00 previously carried by the Society and is in accordance with recommendations made in the last audit.

Insurance policies with the Continental Insurance Company cover stock of periodicals and Journals.

Policy Number	Effective Date	Term	Amount	Coverage
SL571	8/7/48	3 yrs.	$5,600.00	Sprinkler damage
OC9047	8/7/48	3 yrs.	5,600.00	Fire, lightning, and extended coverage

The two insurance policies carry a 90% co-insurance clause.

At the time of the audit, 2 (two) recommendations were made by the auditor. 1. "That the fidelity bond of the Treasurer be increased," as stated above; this has been effected. 2. "That the accounting system be revised to reflect the receipts and disbursements by classification in a columnar journal." The system recommended has been put into effect by the treasurer.

COMMENTS:

From a casual review and comparison with the previous year, it would appear that the Society is $2,500.00 richer than last year. This apparent increase in cash balance is more than represented in the collections on the 1951 accounts. The collections on the 1950 accounts prior to December 31, 1949, amounted to $3,022.56. Whereas, the collections on the 1951 accounts prior to December 31, 1950, amounted to $5,919.78.

During the year there was an increase in collections from subscriptions, but there was a decrease in collections from sale of back issues.

During the year there was an appreciable increase in the cost of publishing the Journal. This increase amounted to approximately $1,500.00 (including, however, a non-recurring item of $1200 for the ten-year index). The forecast for printing the Journal during 1951 is that the cost will be even higher.

The budget for 1949–50 provided for $1,000.00 in the Trust Fund. The Trustees, Council, previous Treasurer and the present Treasurer have not moved towards investing any part of that amount during the year. Action should be taken to clarify that point.

The sale of the 10-year Cumulative Index to date has amounted to slightly over 500 copies. One thousand, two hundred copies were printed. From the Treasurer's viewpoint, it appears that the membership has been slow in reacting to the availability of this valuable companion to the Journal. Many members have been following their dues payments with the additional and subsequent payment for the Index, requiring considerable extra work on the part of the Secretary and Treasurer.

Your Treasurer has not had an audit made for the eight months in office. A true audit from a firm of Certified Public Accountants requires the determination by the audit firm from each member and subscriber as to the validity and accuracy of the past payments. Costs for a true audit appear to be prohibitive. Several audit firms have recommended that for the purpose of the Society's business, an inspection and certification of the fiscal records is sufficient. Previous Treasurers have followed this very practice. The same firms recommend the formation of an audit committee of the Society which would provide the same review and inspection of the Treasurer's accounts.

Respectfully submitted,
/s/ Walter A. Gresh
WALTER A. GRESH, *Treasurer*

REPORT OF THE BUSINESS MEETING OF THE COUNCIL

The Council convened at 7:30 p.m. on March 4 in the Schroeder Hotel in Milwaukee, Wisconsin. Council members in attendance were Ian McT. Cowan, President; D. I. Rasmussen, Vice-President; Shaler E. Aldous, Secretary; Walter A. Gresh, Treasurer; Durward L. Allen, Representative for Region 1; Leonard E. Foote, Acting for Region 2; Thomas R. Evans, Region 3; Lee E. Yeager, Region 4; W. C. Glazener, Region 5; Ben Glading, Acting Region 6 and C. H. D. Clarke, Acting for Region 7.

Many items of business were discussed and passed on by the Council and are herein reported in brief form.

W. B. Davis was appointed as Representative in Region 5 to replace Lee Arnold who has been transferred to South Dakota.

The present membership blank which is antiquated by the new single membership classification was revised to meet current needs.

Section 5 of the By-Laws was amended to have the fiscal year start March 1 and end the last day of Febru-

ary. This was done to coincide with the present period of office being held by the officers of the Society. This will mean that the next fiscal year will embrace 14 months as the reports and business this past year ended on December 31.

The Council approved a budget of $11,790.00. This is to cover printing of 5 issues of the Journal of Wildlife Management and Newsletters, and other costs including reprints, stationery and printing, postage, express, office equipment, clerical assistance, editors expense, contributions and unforeseen miscellaneous expenses. An auditing committee consisting of Ben Glading as Chairman and Lee Yeager was appointed by the President to audit the books of the Treasurer.

Gustav A. Swanson, the present editor of the Journal of Wildlife Management was reappointed for another year. Dr. Swanson was further authorized to appoint Dr. O. H. Hewitt as associate editor to assist in the editorial duties. Dr. Swanson was also directed to obtain fire, lightning and extended coverage, as well as sprinkler damage insurance with a 90 percent co-insurance clause on the journals and indexes stored with the Cayuga Press. (Done. *Ed.*) Henry Mosby was reappointed editor of the Wildlife Society Newsletter.

The Awards Committee was authorized when desirable to present two literary awards, one on fishery work and another on terrestrial wildlife.

The council approved unanimously to affiliate with the International Union for the Protection of Nature. The annual dues for the society are $50.00.

The Constitutional Revisions Committee presented a general overhauling of the Constitution primarily to clarify the language contained therein. Only two deviations in policy were presented. These were (1) to permit a member to become a life member by paying $100.00 either as a lump sum or not less than $25.00 per year over a period not to exceed 4 years. (2) That the president and vice-president be eligible to succeed themselves for a period of not to exceed a total of 3 consecutive terms of 1 year each. The revisions as presented were approved by the Council and will be presented on a ballot to the membership for their votes.

A motion to establish a society library to be deposited in one of the University libraries was defeated.

A proposal to appoint a committee to aid in the preparation of a Wildlife Conservation handbook was defeated. The Council felt that such a book covering the field of Wildlife for the whole country was impractical.

The Council approved the designation of a committee to investigate the possibility of obtaining translations and access to them of pertinent wildlife publications in all foreign languages.

A proposal to supply the Journal of Wildlife Management to several foreign wildlife research agencies free or on an exchange basis was defeated on the grounds that it would set a precedent so that similar requests would have to be granted to more agencies than could be afforded by the Society.

Resolutions presented by the Resolutions Committee were acted upon and those approved by the Council will be presented for action by the members at this annual meeting.

Respectfully submitted,
SHALER E. ALDOUS, *Secretary*

MINUTES OF THE ANNUAL MEETING OF THE WILDLIFE SOCIETY

The fifteenth annual business meeting of the Wildlife Society was called to order by President Ian McT. Cowan at 8:00 p.m., March 5, 1951, in the Schroeder Hotel, Milwaukee, Wisconsin. Approximately 150 members were in attendance.

The reading of the 1950 minutes of the Council and business meeting were dispensed with because both were published in the July 1950 issue of the JOURNAL OF WILDLIFE MANAGEMENT. The minutes of both meetings were approved as printed.

The reports of the Secretary and Treasurer and the minutes of the Council meeting held the night before were approved as read.

Regional reports were read or reported upon and approved as given. They are included separately following this report.

Dr. Warren W. Chase presented a proposition to set up a Society library which could be housed at some university and the facilities made available to all members through the mails. The University of Michigan would be willing to supply space and personnel to handle such a library and its service work. Several members spoke favorably for such a move and one further recommended the microfilming of certain rare publications. Some were hesitant due to the possibility of creating jealousies between schools as to which would get this library or the possibility of one university using this affiliation with the Society for promotional purposes. This proposition was tabled and will be further investigated this coming year and reported upon at the next annual meeting.

Votes of thanks were expressed to the Index Committee, consisting of Daniel Leedy, George Petrides, and Charles Dambach, for their good work in getting out the Ten Year Index, and to Chester Phelps for his splendid job in arranging the technical program of the conference. A standing vote of appreciation was extended to Shaler E. Aldous, the outgoing Secretary, for his efforts in taking care of the Society business during the preceding two years.

C. R. Gutermuth was appointed as new trustee to replace Herbert L. Stoddard whose term expired.

Harold Titus, outstanding wildlife writer, was unanimously elected to honorary membership in the Society in recognition for his work in promoting conservation.

It was unanimously voted to present Mrs. Aldo Leopold with a Leopold Medal in honor of her late husband.

C. R. Gutermuth presented the functions of the National Research Council to the group. A motion from the floor for the incoming President to appoint a member to represent the Society at the Council's meeting was proposed and passed.

The resolutions committee presented three resolutions that were passed. These were: (1) to express thanks to the U. S. Fish and Wildlife Service for its service in putting out WILDLIFE REVIEW and request a continuation of this service and, if possible, to enlarge it to include English abstracts of pertinent foreign publications in the wildlife field; (2) to urge departments of con-

servation of those States in which remnant wolf populations remain, to adopt measures designed specifically to conserve the remaining wolf population; and (3) that the new Secretary be instructed to express the Society's thanks and appreciation to the Wildlife Management Institute, to Robert McCabe and his local committee on arrangements, and to Ian McT. Cowan, retiring President, for their respective parts in making this conference such a splendid success.

Respectfully submitted,

SHALER E. ALDOUS, *Secretary*

ANNUAL REPORT OF SOUTHEASTERN SECTION OF THE WILDLIFE SOCIETY

The last meeting was held in Richmond, Virginia, on October 17, 1950. President Leonard Foote presided and George Moore acted as secretary.

The minutes of the last meeting were read. James Jenkins, Chairman of Membership Committee, reported forty new members and Ned Thornton, Chairman of Program Committee, expressed appreciation for cooperation received in preparing the program.

Dues in the amount of fifty cents (50¢) annually was approved by the group and the sum derived therefrom would be used to defray expenses of the secretary.

Dr. H. S. Mosby reported approximately 200 Society members in the Southeast.

It was favorably voted that the chairman of the Program Committee should request 200 copies of each paper given at the meeting, to be distributed as follows: 150 copies to individuals attending the meeting and 50 copies to the secretary who would make them available upon request to any section member in good standing. It was decided that one volume of all papers presented should be prepared and retained by the secretary as a matter of record.

Election of officers resulted in electing Willis King as President; Earle Frye, Vice-President; and Edward Hueske for Secretary-Treasurer. A vote of thanks was given to retiring officers. Dr. King suggested that consideration be given to holding the meeting at a later date next year.

EDWARD E. HUESKE, *Secretary-Treasurer*
Southeastern Section, Wildlife Society

ANNUAL REPORT OF REGION III OF THE WILDLIFE SOCIETY

The most important activity to report in Region III is the work of the Minnesota Section. At the meeting in San Francisco I reported on the preparation and submittal to Governor Youngdahl by the Minnesota Section, of a proposed game and fish management policy. Receipt of this policy was acknowledged by the Governor but that appeared to be the end of it. However, during the studies made by the 'Little Hoover Commission' ex-

istence of this policy statement came to the attention of the Commission and interest in it has been aroused. Legislators have asked for it as have members of the Natural Resources Committee of the Minneapolis Chamber of Commerce. It has been widely publicized in the Twin Cities newspapers. It has been well received, for the most part, and has the backing of many sound lay conservationists.

More recently the Governor appointed an Advisory Committee on Water Management to develop a water management policy for the State. The Section attempted to get adequate consideration of wildlife resources into the policy. Due to delay in getting under way on the project we did not succeed too well. However, in the final report The Wildlife Society's work was noted for the information of those who will continue the study.

Little was done by way of getting new members. Frankly, that is a somewhat discouraging business. Too many, at present outside the fold, see no advantage in joining, particularly if they already have access to the Journal. In the eyes of some this is the only worthwhile thing the Society does, namely publish the Journal. They ask, "What good does the Society do? How can it benefit me to join?" It is difficult to answer these questions in a tangible way since, as an organization, we do little by way of positive action programs.

It seems to me the Minnesota Section action demonstrates that, as a group, we can influence policies and do it on a high plane. Until The Wildlife Society gets into an action program that can definitely help wildlife management and the profession we have little to offer prospective members.

Respectfully submitted,
/s/ Thomas R. Evans
THOMAS R. EVANS, *Regional Represenative, Region III*

ANNUAL REPORT OF REGION IV OF THE WILDLIFE SOCIETY

Activities undertaken by The Wildlife Society in Region IV, regardless of year or the times, are governed in part by the vast area of the seven states involved. Only Region VII, Canada, is larger. The 230-odd members of the Society residing in Region IV are widely separated, even though a considerable number of them are concentrated in the Denver-Fort Collins and Billings, Montana areas.

Despite the limiting factor of distance, however, several partial get-togethers of the Region membership occurred last year. One such occasion was the Third Northwest Wildlife Conference at Jackson, Wyoming, March 30–31, when Society representatives from all Region IV states were present. No special Society program was scheduled, but one or more members from each state participated in the Conference sessions. Members in attendance at this meeting were estimated at 65.

The second, and perhaps most important, assembly of the membership in 1950 was at the two-day Game Management meetings of the Colorado Game and Fish Department, Denver,

April 9–10. On this occasion, E. R. Kalmbach and R. R. Hill organized a Wildlife Society banquet on March 9, followed by a full evening program featured by a discussion of deer-census methods led by Harold W. Steinhoff. About 30 members of the Society, and half as many visitors, were present.

The third and fourth meetings were on February 25, 1950 and February 24, 1951 at the Foresters' Day program, Colorado A and M College. To these meetings, all members of the Forest Service, the Range Management Society, and the Wildlife Society in adjoining states and at large are invited, and the joint programs presented are of interest to all. The Foresters' Day speaker in 1951 was Edward P. Cliff, Regional Forester, Region II, who gave a confidence-building address on the Forest Service's over-all program in the Rocky Mountain empire. Foresters' Day meetings in both 1950 and 1951 were attended by an estimated 30 members of the Wildlife Society and twice as many foresters, range technicians, and students.

Finally, the undersigned is appreciative of numerous courtesies and much assistance on the part of the membership during the year. In this respect, E. R. Kalmbach, R. R. Hill, and J. V. K. Wagar, were particularly helpful.

Respectfully submitted,
/s/ Lee E. Yeager
LEE E. YEAGER, *Regional Representative, Region IV*

ANNUAL REPORT OF REGION V OF THE WILDLIFE SOCIETY

Society members in Region V continue to remain relatively few in numbers, in comparison to other more densely populated regions, but there appears to have been some increase through the past year.

No regional or sectional meeting was scheduled. A majority of members replying to inquiries from the Representative questioned the wisdom or feasibility of trying to arrange such sessions. However, Oklahoma, New Mexico and Arizona had delegations attending technical sessions of the Western Association of Game and Fish Commissioners. Members from Texas participated in similar sessions of the Southeastern Association.

An important development of the past year has been an extended and intensified program of resource use education channeled through educational agencies of several states into secondary schools and colleges, including teacher training institutions. Society members have been active in this work, with encouragement and support from others.

There is still further evidence that colleges offering professional training in wildlife management are graduating more men than can hope to find immediate employment in that field. Barring disruption of employment by military influences, graduates will need to seek places in a number of related fields. This conclusion is based on observations and reports from various employment

applicants, college representatives and conservation agencies.

During the year, the regional representative prepared and sent out informational and suggestive letters to all known members in Region V, relative to constitutional amendments, and solicited new membership among younger technicians.

Respectfully submitted,
/s/ W. C. Glazener
W. C. GLAZENER, *Regional Representative*

MORE STOCK-TAKING

REPORT BY THE WILDLIFE SOCIETY EMPLOYMENT COMMITTEE

Wildlife Society members will recall the report of last year's Employment Committee published under the title, "It's Time to Take Stock," in the JOURNAL OF WILDLIFE MANAGEMENT, 14 (2): 237–242. The present Committee deemed it advisable to conduct a survey similar to that made last year to determine the number of wildlife students registered at various colleges and universities, the number of wildlife graduates, and to collect data relative to employment. Questionnaries designed to obtain this information were sent to 46 colleges and universities known to be offering wildlife or closely related courses. Replies were received from 45 of these schools scattered throughout the United States, Canada, and Alaska. The list of schools contains 14 institutions not canvassed last year. Although most of these additional schools offered wildlife and related courses last year, the number of schools offering such work continues to increase. Following the survey, we learned of three other institutions offering wildlife training, and there are probably still others of which we are not aware.

Relatively few of the colleges and universities grant degrees in wildlife or conservation, the degrees usually being granted in agriculture, forestry, or arts and sciences. The conception of what constitutes "wildlife majors" and "wildlife positions" varies decidedly at the different schools, hence certain discrepancies undoubtedly occur in listing the numbers of wild-life students and graduates in this report. At many of the colleges, students do not declare their major course of study until the Junior year; therefore, the 1,406 undergraduates listed as enrolled at 45 colleges and universities (Table 1) represent less than the actual number registered in wildlife or related curricula.

A study of the enrollment by classes (Table 1) would indicate that the peak of enrollment has been reached, at least for the present. Of 20 schools reporting undergraduate enrollments for all four classes at the beginning of the 1950 fall term, the breakdown according to classes was as follows: freshmen, 21 percent; sophomores, 28 percent; juniors, 25 percent; and seniors, 26 percent. Some students transfer as upper classmen from other curricula to wildlife, but normally, freshman classes are largest. The survey indicated a minimum of 1,420 undergraduates at 45 schools compared with 1,463 students reported by 32 schools last year. A reduction of 12.7 percent in undergraduate enrollment was indicated by

TABLE 1.—WILDLIFE STUDENT ENROLLMENT BY CLASSES AT THE BEGINNING OF THE FALL 1950 TERM IN VARIOUS NORTH AMERICAN SCHOOLS HAVING WILDLIFE COURSES

Name of Institution	UNDERGRADUATES				Total known Undergraduates	GRADUATES	
	Freshmen	Sophomores	Juniors	Seniors		Masters	Ph.D.
Alabama Poly. Inst.	5	6	5	8	24	14	0
Alaska, Univ. of (1)					20	3	
Arizona, Univ. of	1	1	5		7	0	0
British Col., Univ. of			6	4	10	11	5
California, Univ. of (Berkeley)			30	22	52	6	5
California, Univ. of (Davis) (2)							
Colorado A & M Coll.		10	17	11	38	22	0
Connecticut, Univ. of			10	3	13	0	0
Florida, Univ. of			5	2	7	0	0
Georgia, Univ. of			20	15	35	2	1
Humboldt State Coll.	19	25	21	23	88	0	0
Idaho, Univ. of						8	0
Illinois, Univ. of (2)							
Iowa State College (3)			5	7	12	16	4
Kansas, Univ. of (1)					5		
Louisiana State Univ. (4)							
Maine, Univ. of		14	6	7	27	5	0
Massachusetts, Univ. of	14	9	11	7	41	6	1 (Sp.)
Michigan State Coll.			20	24	44	26	3
Michigan, Univ. of	6	13	8	24	51	6	11+
Minnesota, Univ. of	20	13	19	23	75	20	2
Missouri, Univ. of	5	10	19	20	54	19	3
Montana State Coll.	8	4	11	11	34	12	0
Montana State Univ.	5	21	21	18	65	7	0
Nebraska, Univ. of (5)			16	20	36	0	0
Nevada, Univ. of	3	6	1	3	13	2	0
New Hampshire, Univ. of (6)		3	4	5	12	0	0
N. Y. State Coll. Agr. (Cornell)	17	10	14	10	51	14	12
N. Y. State Univ. (Syracuse)						15	3
North Carolina State Coll.	6	11	4	5	26	4	1
Ohio State Univ.	5	14	18	16	53	12	4
Oklahoma A. & M. Coll.	7	22	0	4	33	4	11
Oregon State Coll.	40	59	41	33	173	18	0
Penn. State Coll.						15	0
Purdue University	2			4	6	1	
Rutgers University (7)				2	2	1	1
South Dakota State Coll.	12	9	10	9	40	0	0
Southern Ill. Univ.	6	3	4	4	17	10	0
Texas A. & M. Coll.	23	22	18	14	77	6	4
Toronto, Univ. of (8)		10	2	2	14	18	16
Utah State Agr. Coll.	16	21	25	20	82	20	1
Vassar College			2	2	4	1	0
Va. Poly. Inst.						5	1
Wash. State Coll.		4	10	7	21	7	1
Wis. Univ. of (9)		1	8	5	14	14	9
Wyoming, Univ. of	7	13	8	16	44	9	0
Totals	227	334	424	410	1420	359	99

(1) Number of undergraduates not designated as to class.
(2) No strictly wildlife courses offered.

Continued on following page.

TABLE 1.—(Continued)

(3) Does not include students majoring in other fields but taking one or more courses in wildlife.
(4) No reply received.
(5) One course in wildlife management offered primarily for students majoring in soil conservation.
(6) Wildlife students in Forestry Department only; does not include zoology majors in wildlife.
(7) Includes students working with shell fish; wildlife curriculum being developed.
(8) Does not include undergraduates in the Faculty of Forestry who go into fish and wildlife work via the Department of Zoology as graduate students.
(9) Undergraduates listed, all in fish work in Department of Zoology; graduate students include both fish and wildlife students in Departments of Zoology and Wildlife Management.

14 schools for the fall term, 1950, as compared with 1949. The increasing number of schools offering wildlife work, however, partially offsets the decrease in enrollment of the older wildlife schools.

In addition to the 1,420 undergraduates registered at the 45 schools, there were 359 Master's candidates and 99 Doctoral students making a total of 1,878 wildlife students compared with a total of 1,784 reported last year by 32 schools. Undergraduates outnumbered graduate students 4.6 to 1 in 1949 as compared with 3.1 to 1 in 1950. Both the holders of Bachelor's and Master's degrees returned for further schooling in slightly greater numbers in 1950 than in 1949, indicating that the trend is for more advanced degrees. This may be due, in part, to some students who fail to obtain wildlife positions, turning to more schooling thus taking advantage of their G. I. Bill education benefits.

During the school year ending September 30, 1950, 488 Bachelor's, 119 Master's and 26 Doctoral degrees were granted to wildlife majors by the 45 schools reporting, a total of 633, as compared with 369 graduating from 32 schools last year. For comparative purposes it may be of interest to note that degrees granted at 22 accredited forestry schools in the United States for the calendar year 1950 numbered 2,039 undergraduate degrees, 263 Master's degrees, and 29 Doctor's degrees. These figures were compiled by Professor Cedric H. Guise of Cornell University.

Slightly less than one-half (44 percent) of the Bachelor degree men who had wildlife majors obtained employment in the wildlife field in 1949–50, as compared with 54 percent obtaining wildlife employment the previous year. Twenty-nine percent returned to school, nearly 5 percent were taken into the military services, and 22 percent found other employment or their status was unknown.

The Master's degree men fared better, 72 percent obtaining wildlife employment as compared with 89 percent last year. Approximately 13 percent returned to school, 3.4 percent joined the armed forces, and the rest obtained other types of employment or their status was unknown to the university professors answering the questionnaire.

Of the 26 Doctor's degree men, 19 (73 percent) obtained wildlife employment, as compared with 90 percent last year; 11.5 percent were taken into military service, and the remainder went into other types of work, including teaching.

Of 328 graduates obtaining wildlife employment, 241 were employed by state or provincial conservation agencies, 62 by Federal agencies, 19 by private individuals or agencies, 4 by universities, and 2 foreign students who had come to this country for wildlife training returned to work for government agencies in their own countries.

Three hundred and twenty-three graduates working in the wildlife field were placed, according to their main duties, in the following categories: Game management or research, 159; fish management or research, 114; both game and fish management or research, 13; administration, 11; law enforcement, 22; and wildlife teaching, 4. It is probable that more of the graduates went into the latter field but since this category was not specifically included in the questionnaire, they were omitted from the job classification, together with a few who went into scouting, museum work, and other fields.

As these graduates gain experience more of them will fill administrative positions. During the year, many appointments to such positions were made from the ranks of technically trained "wildlifers." In at least one instance, a trained technician and conservation professor was appointed chief of a leading state fish and game division, two wildlife professors were selected to head up their respective departments of zoology in prominent land-grant colleges, and the Alaska Cooperative Wildlife Research Unit Leader was named Dean of the Faculty at the University of Alaska, effective July 1, 1951.

At the date of this writing there is still some uncertainty about the draft status of 18-year-olds. If they are drafted into the armed forces, as appears likely, the enrollment of males in colleges will decrease. During a January meeting of the Association of American Colleges, Dr. Guy E. Snavely, Executive Director of the Association, is reported to have predicted a drop of 80 percent in male enrollment by the end of June, 1951, if 18-year-olds were drafted and those in school forced to interrupt their schooling. Other educators predicted a drop in enrollment of from 15 to 35 percent.

Although there would appear to be sufficient numbers of wildlife majors graduating at present to fill most wildlife vacancies, the situation is being altered by the national emergency and unsettled world affairs. The number of students receiving wildlife training will probably decrease and many wildlife positions now filled will be vacated by wildlife managers and conservationists called into the armed forces. Passage of the Dingell-Johnson Bill, August 9, 1950, will undoubtedly create openings in fishery work from July 1, 1951. Even before the enactment of this bill, the wildlife professors indicated that fish students were more successful in obtaining positions than game management majors. Obviously, however, the number of positions established as a result of the Dingell-Johnson Act will fall far short of the number of Pittman-Robertson positions created, due to the greater amount of funds available for the P-R program.

Of 38 wildlife professors expressing an opinion concerning wildlife employment opportunities in 1950 as compared with 1949, 9 indicated that wildlife employment opportunities had improved, 18

stated that they had deteriorated, and 11 had noticed no particular change. Although competition for wildlife positions was undoubtedly keener in 1950 than for several years previously, wildlife graduates fared as well as students in many other fields. From the standpoint of wildlife administrators, opportunities for employing better trained men had improved and the wildlife field had benefited.

Wildlife professors suggested several possible methods for broadening the employment base and increasing the success of graduates in obtaining wildlife positions. Among these suggestions were that conservation employment agencies keep the wildlife training institutions informed of their needs; that pressure be exerted to improve the quality of conservation department staffs; that more combination fish and wildlife training be made available to students; that higher standards be maintained in the training institutions with all prospective graduate wildlife students being carefully screened and the enrollment limited; that greater emphasis be placed on management of hunting preserves, law enforcement, journalism, administration, writing and speaking ability, and public relations; that increased attention be given to economic and sociological aspects of wildlife conservation to encourage development in these fields; that Federal and State Civil Service Commissions be induced to allow more credit for field work performed in obtaining advanced degrees and to permit conservation agencies to employ personnel at salaries sufficiently high to get men with desired training and experience.

As was expressed by one of the wildlife professors in returning his questionnaire, an excessive number of graduates will automatically cause the employment base to broaden because many graduates will be able to see new niches and make new jobs both for themselves and others.

It is presumed that an increasing percentage of wildlife graduates will go into conservation officer positions with state conservation departments where they will engage in law enforcement, public relations, and game and fish management. According to the Wildlife Management Institute's "Administrative and Functional Analysis of State Fish and Game Departments," totals of 3,013 and 1,169 were employed in enforcement and in technical work respectively by 48 state departments as of 1948. Since only 22 of the 1949–50 wildlife graduates were reported as going into enforcement work, it is obvious that with the turnover in personnel expected in this large group, few of the replacements are being made by college-trained wildlife students. Although enforcement personnel outnumbered technicians nearly 3 to 1, in the Institute's compilation, the ratio with which wildlife graduates accepted employment as enforcement officers or technicians was approximately 1 to 13.

Outdoor writing holds promise to wildlifers having writing ability. According to the 1950 International Year Book of *Editor and Publisher*, there were 1,875 daily newspapers in the United States and Canada. Many of these papers have no regular wildlife columns. Many have poorly written columns. In this field, as in conservation education, scouting, biology teaching, nature counselling, and wildlife photog-

raphy, there are opportunities for men having wildlife training and experience.

In the struggle to maintain our natural resources, it would appear that well-trained biologists could serve effectively in many types of natural resource conservation programs, complementing or supplementing work being done currently by agronomists, range managers, foresters, and sanitary and civil engineers.

I. T. BODE
FRANK EDMINSTER
BEN GLADING
J. V. K. WAGAR
DANIEL L. LEEDY, *Chairman*

REVIEWS

Life history, ecology, and range use of the pronghorn antelope in Trans-Pecos Texas, by Helmut K. Buechner. American Midland Naturalist, 43(2): 257–354. March, 1950.

Volumes have been written about the pronghorn antelope; however, this modest report from Texas seems to present more new information about the animal, and in fewer words, than anything published previously. The period of field work was short (June, 1946, to September, 1947) but obviously it was well spent. Financial backing came from the Texas Cooperative Wildlife Unit at Texas A and M College. The report itself ultimately was presented as a Ph.D. thesis at Oklahoma A and M College.

Nearly all aspects of antelope life history and ecology are touched upon, but particular stress is laid on food habits and evaluation of food competition with domestic livestock. The author establishes the fact that antelope are primarily forb-feeders, consuming only moderate amounts of browse and very little grass. Food habits were studied by "time-observation" methods (results expressable in "antelope-minutes of use" of various forage plants) and also by stomach analyses. On the basis of 3,044 antelope-minutes representing 4,956 observations the author concludes that the year-long diet of west-Texas antelope is comprised of about 66 per cent forbs, 30 per cent browse and 4 per cent grass. Most of the forbs eaten are unimportant cattle foods and some of them are actually poisonous to cattle. Hence there is little overlap (estimated 19 per cent) in cattle and antelope diets and negligible competition. Conversely, antelope and domestic sheep share many food preferences, the overlap in diets being at least 33 per cent. Most of the range-land in the Trans-Pecos is heavily grazed and in fact 80 to 90 per cent of it is overgrazed according to the author. Yet antelope do well on many cattle ranges because of the lack of direct competition. They do not do well on sheep ranges and were actually observed to die of malnutrition when confined in heavily grazed sheep pastures. These observations are of real importance in antelope management throughout the west since one of the most widespread complaints among ranchers is that antelope compete with domestic stock. The sheepmen have something of a case but cattlemen apparently do not, for Buechner computes that it requires 47 antelope on the range to consume the food of one cow.

A guarded and objective discussion of predator relations is presented. Coyote and golden eagles are both known to catch some antelope in Texas and the steady increase of antelope over the past two decades is popularly assumed to be a direct result of coyote and eagle control. Buechner grants that control doubtless speeded the increase but points out that better game law enforcement and in more recent years increased rainfall may have been equally important or more so. At several points in the report he states flatly that range conditions, as determined in part by livestock grazing and in part by current rainfall, are paramount in determining average density and productivity of antelope populations, the implication being that predators are of less importance than range. This point of view corresponds with most of the recent findings on deer populations.

The whole report is well written and easy to read, possibly excepting the unnecessarily de-

tailed description of the physical and biotic environment of the Trans-Pecos. All in all, it will stand as a classic piece of work on the pronghorn.—A. STARKER LEOPOLD, *Museum of Vertebrate Zoology, Berkeley, California.*

Water, Land, and People, Bernard Frank & Anthony Netboy; xviii+330 pp., Alfred A. Knopf, 1950. $4.00.

Water problems have become increasingly prominent in America's conservation consciousness in recent years. The ever-growing and controversial big dams programs of the Army Corps of Engineers, Reclamation Service and T. V. A., the increasing frequency and magnitude of floods, water shortages for industrial and civic uses are some of the centers of this interest. Much has been written about water conservation lately and much more will be written in the years just ahead.

Water, Land and People is a book about water. The land and people come in because the authors—a forester and a professional writer—are keenly aware of the inseparability of land and water problems and of the vital control modern man holds over both. Their main point is a plea for a watershed approach to water problems—watershed planning and watershed treatment. They emphasize that our water problems are going to worsen unless the watersheds are properly cared for in addition to the development of water control facilities on the major streams.

The history of land and water misuse and deterioration and the increasing demands for water are described. Deforestation and an exploitive agriculture have been followed by accelerated soil erosion and reduced water holding capacity of the land. Extremes of drouth and flood are more frequent as a result of the loss of water stabilizing ability of the watersheds. Growth of cities has vastly increased the per capita consumption of water at the same time that the population itself was rapidly rising. Demands for irrigation water, hydro-electric power, improved navigation, and flood control spurred the large reservoir building of the Federal Government. Increased demands and reduced availability have combined to make water a critical commodity.

Solutions are either engineering or land use measures or a combination. To date they have been almost all engineering in actual practice. Some 93 per cent of past and currently proposed Federal expenditures in this field have been for engineering works while the other seven percent covers watershed treatments, fish and wildlife developments and recreation.

Discussions of water programs necessarily resolve in considerable measure to an appraisal of the policies and work of public agencies in the field—the Corps of Engineers, Reclamation Service, TVA, Soil Conservation Service, Forest Service, Fish & Wildlife Service and other Federal agencies and more local organizations such as the Muskingum Conservancy District, the soil conservation districts and others. The authors accept rather fully the value of big reservoirs for water conservation but decry the relative lack of accompanying watershed treatment which is equally important. They do point out that more care needs to be taken in deciding on reservoir locations since valuable agricultural and recreational assets are often lost when valleys are flooded. They are critical of the engineers for neglecting to recognize the need for watershed rehabilitation along with the building of main channel structures. The expedient "Pick-Sloan" plan for the Missouri Valley is called ". . . a patchwork of conflicting and some ill-conceived projects . . ."

TVA is praised for its effectiveness in harnessing the main water channels of the Tennessee Valley but is criticized for neglecting watershed treatment. TVA has depended upon demonstrations of soil conservation by selected farmers arranged by county agricultural agents who are mostly paid by TVA. "Unlike the objectives of the soil conservation districts—which TVA has kept out of most of the Valley—this policy does not make for a coordinated attack on sick land and disturbed water flow." The author's solution is for TVA to ". . . become a more aggressive land-restoration and development agency." In general, the authors favor a big stick approach to water problems, advocating regional authorities, control of land use on rural private property, and ". . . a new and centralized agency that will guide the activities of the Federal bureaus." How such an agency will be more effective than the present Department heads, Bureau of the Budget

and other top-level Federal administrative controls is not explained.

Fish and wildlife are mentioned repeatedly as one of the resources affected by water and land problems and the solutions used. A short discussion called "Wildlife Conservation Too" treats briefly of the Federal refuge program and the work under the Pittman-Robertson Act.

Water, Land, and People is well written and deserves careful study by all conservationists. —FRANK C. EDMINSTER, *U. S. Soil Conservation Service, Upper Darby, Pennsylvania.*

Ecology and land-use relationships of small mammals on a Michigan farm. By J. P. Linduska. Game Division, Michigan Department of Conservation, Lansing. x+ 144 pp., 68 figs., 37 tables. 1950. A limited number of free copies are available for those who have special need.

Most ecological studies of small mammals have been concerned with wild or native plants and animals in non-agricultural areas. Now, however, practically all of this nation's rural acreage, except for parks and other recreational sites, is under some form of use-management—whether cultivation, grazing, forestry, or watershed protection. The fact that Linduska made his investigations on farm land makes them of special value to the fields of wildlife management and agriculture, for whenever man uses land certain changes are brought about in the mammal fauna. Whether these changes are desirable or not is a relative matter, however, for what may be one man's agricultural pest may well be another's recreation, or even his livelihood. It is studies such as this one that make it possible to appraise more correctly the effect small mammals have on man's activities and interests.

Linduska spent more than three years at the Rose Lake Wildlife Experiment Station, the principal site for the study. The Station was established in 1939 by the Game Division of the Michigan Department of Conservation for long term studies on the responses of wildlife to conservation farming. It is fortunate that so many different habitats and animal species were all situated in the same general locality, for it makes the comparisons between the different types of land-use of greater significance than if the material had been gathered from many scattered localities where other unrecognized factors might have been operating on the composition and density of the fauna present.

The data presented "include four major considerations: (1) the comparative population levels of associated species of small mammals; (2) the effects of various land-use and farming practices on the numbers of small mammals; (3) interrelationships within the group and with other wildlife species; and (4) species discussions to include such biological information as might contribute to a better understanding of this study and to the ecology of farm wildlife in general."

The commonest small mammal of the cropland habitats studied was the prairie deermouse. This animal was most abundant in fields of corn, wheat, rye, and alfalfa. Red clover appeared to be the least acceptable crop. Dense bluegrass sod was practically uninhabited, whereas annual-weed associations of moderate density supported numbers approaching those in cropland. No conclusions were drawn as to the economic status of the prairie deermouse. With the meadow vole, a frequent pest to agriculture, it was found that periodic invasions of croplands by these animals were aided by field-shocked corn left during the winter and by sod areas that adjoined farm fields.

Considerable information is supplied on the fluctuations of the small mammal populations. Even though the feeding habits of predators were also studied in some detail, no attempt has been made to determine the over-all predator-prey relationships for the study area. Additional information is presented on field and cropland habitats regarding (1) seasonal population trends, (2) small mammal movements and interrelationships, (3) strip-cropping and rodent numbers, (4) populations in field borders, and (5) effects of harvest and cultivation on small mammals.

The second half of the publication is devoted to studies in farm woodland habitats. Fox squirrels showed a marked preference for grazed upland woodlots. They increased in numbers during the three-year study period, whereas the other sciurids decreased—flying squirrels to the greatest extent (by 75 per cent

or more), red squirrels to a somewhat lesser degree, and chipmunks the least. The red squirrel showed a preference for ungrazed lowland woodlots, and chipmunks for ungrazed upland woodlots, but no clear-cut preference for a particular kind of woodland habitat was observed among flying squirrels. The white-footed mouse and particularly the short-tailed shrew were more abundant on ungrazed areas.

The paper bound booklet contains fifty-one good photographs of habitats, small mammals, and predators. The numerous graphs and tables assist the reader to understand readily the data that have been used as a basis for the author's detailed analyses. More than 180 references are appropriately cited and the inclusion of an index of approximately 1,000 page-references enhances the value of the work as a ready reference to small mammal studies. —WALTER E. HOWARD, *Division of Zoology, University of California, Davis, California.*

The Climate Near the Ground. By R. Geiger. Harvard University Press, Cambridge, Mass. xxi+482 pp. 1950. $6.00.

This is a book that should well repay a biologist for the time spent on a deliberate and thoughtful reading. The distribution of plants and animals in space and the distribution of their activities in time are obviously correlated with climate and the temptation to generalize about such correlations is almost irresistible. We note and discuss "life zones", the procession of seasonal events through the latitudes, and the similarity of effects resulting from changes in latitude and in altitude. We even scrutinize weather reports seeking explanations for biological phenomena which we have observed. We do these things even while constantly reminding ourselves and our students, perhaps a little bitterly, that plants and animals do not live in "standard weather shelters." There is a distinctive climate inside of a rodent burrow, a hollow tree, a bird's nest, and even among the hairs or feathers where ectoparasites live; yet we have scarcely begun to acquire the knowledge necessary to draw intelligent inferences about the characteristics of these microclimates.

The first edition of Dr. Geiger's book appeared in 1927 and stimulated healthful interest and caution among many biologists. This new translation of the "amplified second edition" (described in the preface as "the virtual equivalent of the third German edition") should achieve a wider circulation among those concerned with biological problems. This is a *textbook* in an infantile field of study presenting enough of the theoretical meteorologic approach to orient the biologist's thoughts and yet immensely sympathetic (see p. 70) to the biologist's special interest in the cumulated effects of the environment.

One feels that the author's sampling of the biological literature, especially of publications not in the German language, has been superficial but this need not detract from the stimulation of discussions which suggest new perspectives on biological problems. If read in a deliberate manner scarcely a page of this book will fail to raise stimulating questions in the mind of a biologist. Does the very special climate to be found in the one-millimeter thick "skin" of air above the soil account for the curious ability of Collembola to remain active when our thermometer tells us that small insects should not be abroad? What does the low heat conductivity of snow, or the still lower conductivity of leaf litter, mean to the animals which concern us? If radiation reflected by a river (p. 154) can dictate success or failure in a cultivated crop, and if we may descend from forests to "timberline" in a region of cold air drainage (p. 200), have we seen, but not properly observed, comparable situations in the field?

The author understands the biologist's need for meteorologic knowledge and presents a guide to methods, problems, and pitfalls in a field where definitive yet accurate statements are at present impossible.—LAMONT C. COLE, *Department of Zoology, Cornell University, Ithaca, N. Y.*

Estimating Game from Licensee Reports, by William J. Sondrini; Connecticut State Board of Fisheries and Game, Hartford, Connecticut; 1950; Pittman-Robertson Bulletin no. 3, pp. 1–50, tables and graphs.

This bulletin is a study of the game kill reports which were made by licensees in Connecticut for the years 1923–1944, excluding

1939. Data are presented on license sales, percentage reporting, average head of game reported, and estimated total annual kill of game, covering 13 kinds of birds and 14 kinds of mammals. Dates of open seasons and bag limits are listed for 1923–1948. Various factors influencing kill are discussed. Most of the listed game species are considered briefly in the text, with additional information in graphs. Aside from mention of a source of fur price figures, there are no references to other work.

The aspect of this paper which is probably of greatest interest to those concerned with game problems is an attempt to solve, by a new method, the old problem of how much, if at all, licensee reports are biased by a tendency for the more successful hunters to report in greater proportion than the less successful. Of considerable added importance are the historical data on game kill in Connecticut for the period of the study.

Reports of kill for the previous year were made to town clerks at the time of obtaining a license. There was, according to the author, "an evident tendency to report success but not failure." The basic assumption of the method of correction used is that if the more successful hunters report in greater proportion, then over a period of years there will be a relationship between the percentage of hunters reporting and the average reported kill, the less the proportion reporting, the greater the average reported kill.

The correction method in this paper is worked out on the basis of a curvilinear relationship between percentage of hunters reporting and average reported kill in head of game, the latter based upon the total reported kill for all species. The correction reduces the average, and the total, estimated kill according to percentage of hunters reporting, to the level to be expected had 100 percent reported. The same proportional reduction is made for each species, for any year. The estimated total kill always is less by this method than by the usual method assuming no bias.

In considering this paper, it is necessary to distinguish carefully between the potential usefulness of the idea proposed, on the one hand, and the actual execution of the analysis, on the other. To the reviewer, the proposed method of correction seems to be based upon a relationship which is neither simple nor obvious, and therefore to require more thorough study before general adoption. In particular, some independent estimate of the true average kill should be obtained for comparison. Two obvious questions on the method are, first, would not any simultaneous variations in general hunting success and percentage of hunters reporting produce an erroneous relationship; and, second, is the relationship between success and reporting constant for hunters of all game birds and mammals?

Several errors are apparent in the treatment of the data in this paper. Since others may try this method with their own data, some of the mistakes will be discussed here. The conventional single asterisk * will denote a value of r, the coefficient of linear correlation, significantly different from zero at the 5 percent level.

The relationship between percentage reporting and average kill reported has been computed by a rather involved process. The values for the 21 years were ranked according to percentage reporting, averaged in groups of 10 percent range, and then "adjusted" to center each group average precisely upon the midpoint "to make further computations easier." This adjustment of average kill was made according to the proportion between the mean percentage reporting and the midpoint value of the class. It happened, apparently by accident, that this wholly erroneous adjustment resulted in moving most of the five values into somewhat better conformity with the hypothesis of a curvilinear relationship.

The data were next "extended." This meant simply supplying values for 10, 20, 80, 90, and 100 percent reporting according to the supposed trend of the five "adjusted" values. A parabola was fitted to what were now 10 values, the 5 "adjusted" values and the 5 fictitious "extended" values. An index of correlation was computed from the deviations of the values from the parabola, yielding $R = .9922$ for the 10 values and $R = .9664$ for the 5 "adjusted" values.

These numerically high values lend a deceptive air of reliability to the whole computation. The coefficient based upon the 10 values is without meaning since it is based upon 5 known values plus 5 values from extrapolation. The coefficient based upon the 5 known, if "adjusted," values is not significant, with only 2 degrees of freedom remaining after fitting a

second degree curve to a series of 5 values.

If average reported kill is plotted against percentage reporting, for each year, the data are widely scattered, and a coefficient of correlation ($r = -.41$) falls just below the level required for significance. There is evident, however, a curious grouping by years, suggesting that there is some effect of time. The first year of the records, 1923, stands by itself with the lowest percentage reporting and the highest average reported kill. The years 1924–1929 are characterized by a generally high percentage reporting and low average kill. The years 1930–1940 spread over the whole range of percentages reporting but show generally high average kill. The years 1941–1944 are of low percentage reporting and generally low average kill. The individuality of these three groups seems quite distinct, with only a single instance where one point of one group could possibly be considered to fall within the limits of another group. Omitting the year 1923, it is possible to demonstrate a statistically significant relationship ($r = -.53^*$) between percentage reporting and average kill reported, computed about the average regression within these chronological groups. It would seem, however, that any system of corrections should be based upon some more conclusive relationship than this.

This natural ordering of the data into the same chronological groups is clearly apparent in several sets of comparisons, including the average kill plotted against license sales, the percentage reporting plotted against license sales, and the estimated total kill plotted against license sales. In Graph B of the paper, the groupings may be seen by writing in the year next to each plotted point.

If percentage reporting is plotted against time, there are apparent two distinct and regular peaks of high reporting centered about the years 1926 and 1934. Is reporting a cyclic phenomenon among hunters?

Much of the history of pheasant hunting in Connecticut appears to be covered by this study. On Graph E are shown both estimated kill and numbers of cocks and hens released, by years. The author states, concerning this graph, "Changes in the kill follow the changes in number of birds released in 15 of the 23 years. Hence, the pheasant stocking has influenced the kill of the species." This is an important conclusion, if confirmed by the evidence. The emphasis, properly, is on changes in numbers rather than on the absolute levels. Examination of the graph, however, shows that often the changes in releases followed, instead of led, the changes in kill. This impression is confirmed by analysis on the basis of percentage change (compared to the previous year) for both kill and release figures. There is little apparent relation between change in kill of one year and change in releases of the same year ($r = .08$), or change in kill of one year and change in releases of the previous year ($r = .08$), but the relation between change of kill for one year and change in releases for the *following* year was at least significant ($r = .53^*$). Could the changes in rate of release have been determined partly by changes in hunter success?

Fur prices are allowed for by computing a population index as kill divided by price. As the author states, this assumes that kill varies directly with price. There is some discussion of the effect of season length and bag limit upon kill.

This paper contains much useful information, and the author must be credited with having attacked a difficult problem in a new way. The suggested method of correcting for bias cannot be judged fairly without some independent estimate of the true average kill. Until such basic information becomes available, the general problem of bias in hunter reports will remain unsolved. Perhaps the fundamental studies must be made by social scientists, since the problem is one of human behavior.—DON W. HAYNE, *Michigan Agricultural Experiment Station, E. Lansing, Mich.*

Duck Hunter's Dilemma. Sound motion picture in 16 mm. colored film, directed and photographed by W. J. Breckenridge, produced by Audio-Visual Services, University of Minnesota, Minneapolis. Price, $153.

While this department does not usually notice motion pictures, it can be argued that the medium is one form of publication, and the present film is such an outstanding one that it deserves wide recognition. In approximately 800 feet, running about 20 minutes, this film makes a remarkably fine survey of the needs of our waterfowl populations, and particularly

of the research methods being applied to meet these needs. Produced in cooperation with the Wildlife Management Institute, and with the support of James Ford Bell, the film uses the Delta Waterfowl Research Station as an example of coordinated research on this resource being conducted in various parts of North America. While presumably prepared for the general public, particularly sportsmen, the film covers so much that it would be of distinct value to professional classes in wildlife management, because it portrays very effectively a large number of the techniques of waterfowl research: aerial and ground census methods, mass banding of flapper ducks by driving them into net traps, capture of incubating female ducks on the nest by the Lyle Sowls' technique, coloring unhatched ducklings in the egg by the technique recently described in these pages by Charles Evans, fluoroscoping ducks for body shot by the William Elder method, and many others. The film is one of the most effective instructional pictures on wildlife the reviewer has had the pleasure of seeing.—GUSTAV A. SWANSON.

RECENT PUBLICATIONS

A Device for Dating Natural Events in Game Animals, by Cyril Kabat, Donald R. Thompson, and Frank M. Kozlik. Technical Wildlife Bulletin Number 1, Game Management Division, Wisconsin Conservation Department, Madison 2, Wisconsin (Pittman-Robertson Project 9-R). 4 pp. 1950.

Pheasant Weights and Wing Molt in Relation to Reproduction With Survival Implications, by Cyril Kabat, Donald R. Thompson, and Frank M. Kozlik. Technical Wildlife Bulletin Number 2, Game Management Division, Wisconsin Conservation Department, Madison 2, Wisconsin (Pittman-Robertson Project 9-R). 26 pp. 1950.

Waterfowl Populations and Breeding Conditions, Summer 1950—With Notes on Woodcock and Wilson Snipe. By C. S. Williams and others. Special Scientific Report (Wildlife), No. 8. U. S. Fish and Wildlife Service, Washington, and Canadian Wildlife Service, Ottawa. 256 pp. Processed.

This, the fourth in the annual series of reports on waterfowl breeding populations and breeding conditions, is far more comprehensive than any of its predecessors. It consists of 45 separate summaries of waterfowl breeding conditions and populations in the northern states, Canada, and Alaska, plus four summaries of woodcock census studies and one on Wilson snipe singing ground counts in eastern Canada. The reports represent contributions from staff members of the various states and provinces, the Canadian Wildlife Service, the U. S. Fish and Wildlife Service, Wildlife Management Institute personnel, and one from a biologist associated with private duck preserves. That the information presented here is almost entirely strictly quantitative represents a tremendous forward step in the management of migratory birds.

Missouri: Its Resources, People, and Institutions. Noel P. Gist, Thomas C. Morelock, Clarence M. Tucker, W. Francis English (Edited by). 1950. University of Missouri, Columbia, Missouri. 605 pp. $4.50.

This exceptionally fine book on the State of Missouri contains 28 chapters by different authors who are mostly members of the faculty of the University at Columbia, or the University School of Mines and Metallurgy. Included are chapters on the soil, water, minerals, and forests, but the one on "Wildlife", by Rudolf Bennitt and Robert Campbell is of greatest interest here. While Campbell was responsible for the portion dealing with fishes and Bennitt with the remainder of the chapter, both helped edit the entire chapter. In 27 pages the authors have presented an exceedingly useful and readable review of wildlife and its conservation in Missouri, beginning with a historical account, describing briefly the present Conservation Commission organization, and pointing to present trends. Teachers, sportsmen, and in fact all classes of readers from Missouri and nearby states will find this condensed account as fine a syllabus of local wildlife and its conservation as they are likely to encounter.

Notes on Some British and Foreign Wildfowl. By Michael Bratby. Published for the Severn Wildfowl Trust by Country Life Ltd., Tavistock Street, London. 18 pp. 10s. 6d. 1951.

This little book, which contains brief notes on, and colored pictures of 46 species of wildfowl, was prepared for visitors to the Severn Wildfowl Trust as an aid to identification of the collection of live birds maintained there. The five colored plates by J. C. Harrison portray breeding males only. It is interesting that the number of visitors to the Severn Wildfowl Trust has become so great as to require such a book as this.

Severn Wildfowl Trust. Third Annual Report, 1949–1950. Country Life Ltd., London. 130 pp. 10s. 6d.

This exceptionally attractive report will be of interest to waterfowl researchers for several reasons; among them the summary of wildfowl banded and recovered, the brief section on the participation of the Severn Wildfowl Trust in attempts to save the Hawaiian goose (the report includes a beautiful full-color cover illustration of the nene, a page of drawings from life, and six excellent photographs), and four colored plates which portray the eighteen species and subspecies of swans and geese found in the British Isles. Future reports are to cover all British waterfowl. The vigorous program of the Trust is nicely described, and the whole gains much in charm from the many halftones and line drawings by Peter Scott. Copies should be ordered direct from the Severn Wildfowl Trust, The Newgrounds, Slimbridge, Gloucestershire.

Institute of Freshwater Research, Drottningholm.
Report No. 29. Fishery Board of Sweden. Annual report for the year 1948 and short papers. 1949. Pp. 152. kr. 3:30.
Report No. 31. Annual report for the year 1949 and short papers. 1950. Pp. 162.

With these two reports, the Institute of Freshwater Research has initiated a policy of publishing annually a collection of papers representing research contributions of its staff, together with the Annual Report of the Director. Previous separate publications of the Institute since its organization in 1932 are listed in the 1948 report. Twenty-nine of the papers in these two reports are in English, one in French, and one in German. One needs only scan the papers to realize the great value they will have to all workers concerned with freshwater fishery biology, limnology, and biologists generally. One is struck by the fact that references cited show a broad familiarity with American as well as European literature on the subject.

A paper which will be of great interest to students of evolution and fish taxonomists begins in the 1948 collection as "The Coregonid Problem. I. Some General Aspects of the Problem", and continues in the 1949 collection with "II. Morphology of Two Coregonid Species in Different Environments."

Titles of some of the other papers, to indicate the scope of the publications, follow: (1948) "Influence of Heredity and Environment on Various Forms of Trout"; "Movements and Growth of Grayling"; "Predators on Salmon Fry in the River Morrumsa in 1948"; "Experiments . . . on the Influence on Fish of Bomb-Dropping"; "A New Water Sampler"; "Control of Trout Migration by A Fish Ladder"; and in 1949—"Preliminary Report of Certain Experiments With a View to Exploiting Lakes Empty of Fish"; "Heterogeneous Stimulus Summation in the Release of Spawning Activities in Fish"; "The Fitness of Nylon Thread for Manufacture of Fishing Tackle"; "The Possibility of Avoiding Winter Kill of Fish."

Natural Resources Activity of the Federal Government, Historical, Descriptive, Analytical. By J. R. Mahoney. Public Affairs Bulletin No. 76, The Library of Congress, Legislative Reference Service, 249 pp., many tables, graphs. 1950. (Processed) (May be obtained from the Card Division, The Library of Congress, Washington 25, D. C., at the price of $1.70 per copy).

This report was prepared for the Task Force on Natural Resources of the Hoover Commission, and provides a useful review of the policies and activities of the Federal Government in the natural resources field. It is rather general (the Fish and Wildlife Service, for example, is covered in approximately 20 pages), but it does cover all natural resource activities in the several different units of the government devoted primarily to conservation, and as a broad statement, will undoubtedly be useful to persons teaching general conservation courses. In the fish and wildlife field it is no substitute for several other more detailed reports which are available and well known to readers of the Journal.

BRIEFER ARTICLES

AN AERIAL COYOTE-ANTELOPE OBSERVATION

The extent of coyote predation upon antelope kids has long been a controversial subject and, consequently, one of interest. Two on-the-spot observations, of coyotes diligently searching areas likely to harbor antelope kids, have been recorded in northwestern South Dakota. The bones of young antelope have been found near coyote dens. However, the following observation is the first known record of the behavior of antelope does and kids when pursued by a coyote.

A coyote in pursuit of its breakfast was observed by personnel of the South Dakota Department of Game, Fish and Parks during the 1950 West River aerial antelope census. The incident occurred ten miles west of Buffalo, near highway 8, shortly after dawn on the morning of June 4.

What first appeared to be a lone antelope and a bouncing jackrabbit was soon recognized as a doe and a kid antelope being chased by a coyote. The time was noted at 4:45 A.M., and the plane's altitude was estimated at 150 feet. The animals below paid no heed to the airplane. The plane's altitude was increased to about 700 feet, and circling above the contestants, the scene below was observed in its entirety.

At the beginning of the race the doe was leading, followed by the kid; and trailing about 300 yards in the rear was the coyote. After about a mile's chase, the coyote slowly gained until it was within fifty yards of the kid. At this time the doe dropped back behind the coyote, and then with a sudden burst of speed, boldly ran into the side of the coyote, rolling him end over end. Back on his feet, the coyote, constantly worried by the doe, continued in pursuit of the kid. During the next two miles the doe twice upset the coyote in the same manner—running ahead and then behind, criss-crossing the path between the kid and the coyote. After the third upset the coyote veered from side to side to avoid body contact with the doe. During this latter period, the kid slowly out-distanced the coyote and was soon joined by a second doe from the area. The second doe had a kid hidden near by.

The second doe ran with the pursued kid, seemingly to be leading it. The first doe continued to harass the coyote and the race continued. As the contestants passed over the crest of hills, the leading doe and kid would veer off at right angles in an attempt to lose the coyote. After the third such attempt the doe and kid separated, each going off in opposite directions. The coyote, duped by such a maneuver, followed the leading doe; and the kid was left free to slow its pace and seek refuge in a near-by depression.

The coyote, discovering that he had lost sight of the kid, stopped and rested near the crest of a hill, and scanned the area for some sign of the kid. The two does remained in the area and leisurely trotted about keeping within sight of the coyote.

The observers concluded that the race was over; the time was noted to be 5:00 A.M.; and the total course of the chase was estimated to have been five miles.

The coyote was buzzed by the airplane in an effort to drive him out of the area; but he refused to leave and was little frightened by the plane.

The coyote was small and appeared to be in rather poor condition. It may have been a bitch coyote with a litter of pups. The airplane and observers left the scene, leaving the contestants as before—free to continue the race for survival.—WAYNE DAVIS and DONALD PUTNAM, *South Dakota Dept. of Game, Fish, and Parks, Rapid City, S. D.*

Accepted for publication September 5, 1950.

THE EFFECT OF CLIMATE ON PHEASANT RANGE IN HAWAII

The presence of pheasants in the Hawaiian Islands has caused many people to wonder what factors are operating to allow these birds to live in a range that is apparently so different from its range on the mainlands of North America and Asia. The range of pheasants in the Hawaiian Islands covers a far greater variety of climatic combinations than it does on the mainland. They are found there in temperate climates to elevations of over

9,000 feet and they are also found in the subtropical lowlands where year-round temperatures vary between 70° and 80° F on the average. They are also found in regions having a much higher annual rainfall than stations in mainland pheasant range.

In 1948, Graham and Hesterberg set forth the hypothesis that the southern range of the ring-necked pheasant in the United States was dependent upon temperature and precipitation during the period of oviposition. The beginning and ending dates of the critical oviposition period (April 1 to May 31) were plotted on climatographs for stations within the pheasant range and for stations outside of the pheasant range. Rectangles drawn through these points were plotted on each bioclimatograph and then were compared. The rectangles were in much the same place for different stations within the range, and were separated in the comparison with stations outside of the range. This indicated a good possibility of temperature and precipitation as being limiting factors during this critical period. In this study a similar comparison was made between the critical period extremes for four unirrigated mainland stations within the pheasant range and three stations within the pheasant range in Hawaii. Also a comparison was made between irrigated lands in both places.

The rectangle drawn up for the unirrigated mainland stations was based on the bioclimatographs of Bismark, North Dakota, Detroit, Michigan, Portland, Oregon, and Pontiac, Illinois. The Hawaiian stations used were the Haleakala Ranger Station, Maui, el. 7,030 feet, the Kula Sanitorium, Maui, el. 3,004 feet, and the Volcano Observatory, Hawaii, el. 3,971 feet. The irrigated areas used in this study were Merced, California, and Salt Lake City, Utah for the mainland, and Haiku, Maui, el. 485 feet, Kaanapali, Maui, el. 12 feet, and Pahala, Hawaii, el. 850 feet.

The period of oviposition used in Hawaii is longer than for the mainland, extending from March 15 to June 15, except for the Haleakala Ranger Station. For this station, April 15 to June 15 was used.

The Haleakala Ranger Station is the only station within the range of high pheasant density (51 to 100 birds per sq. mi. Schwartz and Schwartz, 1949) where climatological data is available. The rectangle falls nearly all within the rectangle for the mainland stations. This seems to show a good climatological relationship between the two places for the time of oviposition. Because the rainfall varies so much in the same month of different years, comparisons were made year by year for this station. Oddly enough, every rectangle plotted overlapped the rectangle for the mainland stations.

The second best of all the comparisons made was with the Kula Sanitorium station. It is located in an area of medium pheasant density (26 to 50 birds per square mile).

At the lower, subtropical stations used, there seemed to be little relationship between the rectangle comparisons, indicating that factors other than climate are operating to produce low to medium densities of pheasants. Climates seemed both too warm and too wet for successful clutches.

Graham and Hesterberg suggested that heat from the direct rays of sunlight on pheasant eggs will kill the life within them above certain temperatures. In North America, pheasant nests that are built in the spring often lack protection against sunlight because during the winter herbs and grasses die back and are crushed down under snows and most bushes and trees lose their foliage. In Hawaii there is year-round shade protection by grass, herbs, shrubs, and trees because of the perpetual growing season. The foliage acts as a successful insulator against the damaging heat.

The other question, that of higher rainfall, is more puzzling. It will require much study in the field in order to draw any definite conclusions. The only suggestion that I can make at the present has reference to the soil and the underlying bedrock. Because the Islands are young, geomorphically speaking, the soils are generally quite shallow and porous, and the basalts and other volcanic rocks are highly scoriaceous and fractured. Rain water sinks in rapidly and seldom stands in pools for more than a short period of time in the pheasant range. Unless a nest were in the channel of a cloud burst stream, there would be little danger of its being flooded. Similar amounts of rainfall in most mainland areas would be unfavorable towards pheasant populations.

The areas in the Hawaiian Islands that have very high rainfall produce jungle-like cover and are definitely unsuited to pheasants.

LITERATURE CITED

GRAHAM, S. A. and HESTERBERG, G. 1948. The Influence of Climate on the Ring-necked Pheasant. Journal of Wildlife Management, Vol. 12, (1): 9–14.

SCHWARTZ, C. W. and SCHWARTZ, E. R. 1949. The Game Birds of Hawaii.

BENNITT, R. and TERRILL, H. V. 1940. Possible Temperature Factors in North Central Pheasant Distribution. Trans. N. Am. Wildl. Conf., 5:428–432.

U. S. WEATHER BUREAU. Yearly summary of Climatological Data, Hawaii Section.

—RICHARD J. HARTESVELDT, 1057 *Revere Court, Willow Run, Michigan.*

Accepted for publication October 24, 1950.

SPORT FOR PHEASANTS?

An interesting observation was made in the Haleakala Section of Hawaii National Park, Maui on male ring-necked pheasants. During the crowing season several males were apparently engaged in a diving sport. The Haleakala Crater is over one thousand feet deep at the points of observation. The crater walls lie roughly at the angle of repose. The birds would fly from the rim, diving downward about five or ten feet above the ground. Without flapping their wings the birds glide downward at a high rate of speed until they near the crater floor. Then they circle around 180°, still not flapping their wings, and execute a beautiful uphill landing. The process was repeated after some time. Females were not noted in the vicinity.—RICHARD J. HARTESVELDT, 1057 *Revere Court, Willow Run, Michigan.*

Accepted for publication October 24, 1950.

YIELD OF ACORNS FROM A POST OAK, QUERCUS STELLATA, AUGUST 1949, INDIAN TRAIL REFUGE, DENT COUNTY, MISSOURI

Acorns have been known to be included in the diet of many animals and birds for centuries. Game managers, aware of the importance of acorns to wildlife species through food-habits studies, have been devising techniques for sampling and measuring acorns as a crop.

A reasonable approximation of acorn yield can be had by sampling a proportion of the acorn fall of individual trees. When the average production per tree and the number of trees per acre are known, yield per acre can be computed. It is seldom possible for one to know exactly the number of acorns produced by an oak. This opportunity was presented the writer during the course of sampling acorns in the oak-hickory forests of the Ozarks of south Missouri.

A wind and rain storm on August 19, 1949, at Indian Trail Refuge, Dent County, uprooted several trees throughout the area. A fallen post oak, *Quercus stellata*, growing in a park-like upland site was found to have a crop of acorns still intact. All of the acorns visible to the naked eye were removed from the tree in a systematic manner after various dimensional measurements were taken of the tree. The measurements were as follows: d.b.h., 1 foot; height of tree, 49 feet; height of crown, 39.5 feet; greatest diameter of crown, 24.8 feet; height of fork of main trunk, 28 feet. Being green, the acorns were firmly attached to the cup, and therefore data concerning size and weight refer to acorns with cups.

Information gained later from the sampling of acorns from 17 similar post oak trees in the same area gives some indication as to what might have happened to the acorns by December 15, had they fully ripened. Based on the percentages of the acorns sampled, 7,816 acorns would fail to develop. The remaining 2,335 mature acorns (most of the acorns in the 4–3 eights diameter class) represent a calculated potential of 5.8 pounds of food for wildlife. However, 1,168 acorns would be damaged by insects, fungi, etc.; 1,139 (about 2.8 pounds) would be utilized by wildlife; and 28 acorns would be classified as sound and unused. These proportions vary with the abundance of acorns and wildlife in any one year.

TABLE 1.—YIELD OF ACORNS FROM A POST OAK, *Quercus stellata*, AUGUST 1949.

Diameter Class (in eights of an inch)	Number		Weight	
	Number	Per Cent of Total Number	Grams	Per Cent of Total Weight
4 to 3	3,442	33.9%	1,184.5	73.4%
3 to 2	1,385	13.6%	262.0	16.2%
2 to 1	4,544	44.8%	160.7	10.0%
Below 1	780	7.7%	6.2	0.4%
Total	10,151	100.0%	1,613.4	100.0%

The sampling of mast productions wherever used also affords an opportunity to study and compare the actual yield of mast from individual trees with the calculated yield from similar trees. Although impractical as a method by which total mast production can be computed, it does afford a means of accurately measuring the potentialities of given trees as producers of wildlife food. Windfalls and timber operations provide an abundance of material for examinations. When considering a potential mast-bearing tree for study, it is best that (1) the tree be intact and free of other trees or shrubs upon coming to rest on the ground, (2) the mast be developed sufficiently to be seen by the naked eye and yet not fully ripened enough to be dislodged by the fall of the tree, and (3) the observer be persistent in the scrutiny of the tree and the enumeration of the mast it holds.—DONALD M. CHRISTISEN, *Missouri Conservation Commission, Columbia, Missouri.*

Accepted for publication November 8, 1950.

FROGS IN A FARM FISHPOND

In the Southeast farm ponds are steadily gaining in popularity and becoming accepted as a regular part of the farm program. Frog production that is sometimes attained in these ponds has not received much attention. It has been possible to get a quantitative measurement of frogs on one of these ponds in the vicinity of Gadsden, Alabama. Records from this pond indicate to some extent the frog production that can be expected from a small water area.

In the fall of 1943 an Etowah County farmer built a dam across a hollow impounding water over 1½ acres of land. The pond is in his farmyard and under close observation. The near edge is within 200 feet of the house and 100 feet of the barn. A cultivated field is on the far side. The shoreline is 975 linear feet. The nearest "live" water is in a creek one-eighth mile away, while the Coosa River, a large sluggish stream, flows at a distance of a half a mile. This is the only pond in the community.

The pond filled with water in the winter of 1943–44 and was stocked with bluegill bream (*Lepomis macrochirus*) and largemouth bass (*Huro salmoides*) in the fall of 1944. It has been fertilized at irregular intervals in the summers since stocking. Vegetation on one side has been mowed regularly. Fishing was fair in the beginning and poor in later years. The usual catch consisted primarily of undersized bream. The farmer became dissatisfied with these results and in the fall of 1949 the pond was drained. Its contents revealed large numbers of small bream and a few bass as large as 3 pounds.

Frog hunting was carried on from a boat at night with the aid of a spotlight and a gig or rifle. The common bullfrog (*Rana catesbiana*) and the green frog (*R. clamitans*) were the edible species. Table 1 shows the harvest of these two species each year.

In the spring of 1944 many frogs were observed crossing the cultivated fields to this pond from the river and creek and by fall 94 had been removed. That summer these frogs deposited masses of floating eggs around the water's edge. In 1945 this farmer stopped harvesting frogs when 200 had been taken and

in 1946–47–48 he stopped at 250. In each of these years egg masses were deposited and a successful hatch of tadpoles resulted.

TABLE 1.—FROG HARVEST BY YEARS

Year	Number of frogs removed
1944	94
1945	200
1946	250
1947	250
1948	250
1949	40

It has been assumed that for restocking purposes in this area the green frog spends one winter as a tadpole. Recores from this pond indicate eggs of this species were deposited in 1944 and that transformation took place in 1945. In the North the bullfrog spends two winters as a tadpole but in the South it is probable that only one winter is spent in that form.

In the spring of 1948 a city friend gave this farmer 4 domestic ducks. These birds had access to the pond and spent most of their time feeding around the water's edge. They were observed eating frog eggs and it was concluded that they consumed them all, since none hatched. No year-old tadpoles were found in the pond that summer, and these evidently were eaten also. In the spring and summer of 1949 very few frogs were heard around the pond. Killing stopped when 40 were taken. Even this number were difficult to find. It was assumed that most of those killed in 1949 were migrants and had grown elsewhere. The same supposition was made for the 1944 population.

The yield from this pond indicates a maximum frog population attained by the second year. This is logical for all ponds. From the second year on, in ponds managed for fish production, frogs should decrease in numbers as bass would normally become large enough to eat them regularly. Results from this pond indicate there were not sufficient bass of size to consume the frogs. Therefore, frog harvests were near optimum.

Personal correspondence with H. S. Swingle, Fish Culturist, Alabama Polytechnic Institute, Auburn, Alabama, confirms these observations. Swingle also pointed out that cover plants growing in the shallower water would afford protection from bass and as a result frog yields would be higher in "weedy" ponds. His statements and these results might be interpreted to mean that the frog population may be indicative of the status of bass in a pond.

After the ducks were introduced into the pond, frog production dropped to a minimum. A similar reduction would probably have taken place had the bass attained sufficient size and number to consume the tadpoles and frogs.

It is logical to conclude that (1) frog production was successful in this pond where fish failed; (2) production in this pond can be estimated by subtracting the numbers which migrated to the pond in 1944 or 1949 from the yield which was 250; (3) ducks and frogs cannot be produced on the same pond.—WALTER ROSENE, JR., *U. S. Fish and Wildlife Service, Gadsden, Alabama.*

Accepted for publication November 16, 1950.

TALLY COUNTER HOLDER FOR MAKING FIELD OBSERVATIONS

The tally counter is widely used in field reconnaissance and research, whether such use pertains to forestry, botany, wildlife, or other work. Such recording of data often requires alternate use of two or more counters. In such instances great care must be exercised in use of individual counters to prevent mixing, thus inducing serious errors in the information being collected.

In waterfowl studies conducted by the writer for the Colorado Cooperative Wildlife Research Unit, the probability of inaccurate recording was largely eliminated by arranging, in a series held by a wooden frame, the several counters used in field work. This simple device (Fig. 1) not only made for greater accuracy, but increased efficiency and facilitated handling of instruments under working conditions.

Construction of the frame is simple and inexpensive. It is recommended that 1/2″ soft wood and small flat-headed screws be used. Necessary tools are a 1/2″ mortise bit and 1/2″ dado head (for power equipment) and a brace and 1/2″ bit, wood rasp, fine-toothed saw, and screwdriver.

Dimensions for the 3-counter frame illus-

trated (using the Adams Hand Tally Counter No. A-2000) are: Base—3″ x 7-1/2″; top—1-1/2″ x 7-1/2″; dividers—1-1/8″ x 1-3/4″. Two or three coats of spar varnish help prevent absorption of moisture and resultant warping of the frame. Any number of counters may, of course, be aligned in the same manner.

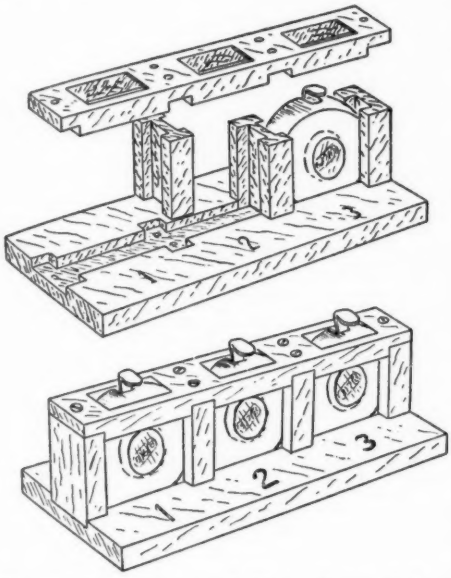

FIG. 1. Tally counter holder. Above, before assembly; below, assembled.

This holding device was used mainly for recording the number and sex of ducks by species. For this purpose it necessitated the use of only one hand, leaving the other free to move and adjust the spotting scope. Recording did not, therefore, require the use of sight to ensure picking up of the right counter, since the three instruments remained at all times in the same position and in a linear distance of less than 6 inches. Counting was thus speeded up because the recorder did not have to alternate the sense of sight in recording and scope operation.

Technicians for the Colorado Game and Fish Department have suggested use of this assembly in aerial big-game work. With 3 counters the observer could record males, females, and young, leaving the pilot entirely free to manoeuver the plane. In present operations the pilot often records one of the sexes or the young with a single tally counter.

Attachment of frame to dash or other convenient position on plane or auto would facilitate use while traveling.—JACK R. GRIEB, *Colorado Cooperative Wildlife Research Unit, Colorado A. & M. College, Fort Collins.*

Accepted for publication May 11, 1950.

SECURING CELLULOID BANDS ON WILDFOWL

Colored celluloid leg bands have been used for a number of years for identification in research, and for miscellaneous purposes on poultry and wildfowl.

The main shortcoming of the celluloid band when used on waterfowl, as reported by many investigators, is that it is sometimes lost.

The writer has applied many celluloid bands on various species of poultry and wildfowl over a period of years with a loss of about one per cent of the bands the first two or three years. This high degree of success is due to the fact that the band is held together by a hollow rivet, and the band cannot be removed from the leg by the bird under any circumstances, with the rare exception of the breaking of the band by catching on an obstacle.

The procedure for applying bands for this use is, first: care should be observed in selecting the correct size of the band to be used on a particular species, then drill a hole (just a little larger than the outside diameter of the rivet) about three-eight of an inch from the end of, and through two layers of the celluloid band. Apply the band to the leg, insert a hollow rivet in the hole, with the rivet head toward the tarsus, then rupture or roll over the protruding hollow part of the rivet on the band with the use of a sealing pliers. Bands of good quality, applied in this manner, will remain on the leg for a period of two to four years. Loss thereafter is attributed to the wear of the material, breakage and falling off.

The bands, hollow rivets, and pliers, are obtainable from any large poultry supply house or the National Band & Tag Company

of Newport, Kentucky. Though the writer has never used them, he believes the solid lead rivets could be used with equal success.

A simple method for the drilling of large numbers of bands involves the use of a slotted brass or iron tube or pipe about fifteen inches long, with inside diameter approximately the same as the outside diameter of the band. Place the bands inside of the tube, insert a dowel rod to fit the inside diameter of the bands and a sturdy drilling jig will result.—CHARLES W. KOSSACK, 715 *South Division Street, Barrington, Ill.*

Accepted for publication Nov. 6, 1950